A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XXII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR, FOR

W. NICHOLSON,

CHARLOTTE STREET, BLOOMSBURY;

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN HILL.

1809.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, the Rev. E. J. Burrow, A. M.; Sir Thomas Clarges, Bart.; P. Barlow, Esq.; Mr. John Farey; Mr. William Skrimshire, jun.; Dr. Thomas Stewart Traill; A Correspondent; Richard Greene, Esq. P.R.M.S.E.; Mr. B. Cook; Mr. James Scott; G. K. M.; Philochemicus; Mr. de Luc; E. F. G. H.; S. N.; Sir George Cayley, Bart.; T.; Mr. Charles Hayter; Mr. W. Weldon; W. Saint, Esq.; Arthur Aikin, Esq.; Thomas Young, M.D. F. R. S.; John Gough, Esq.; J R. Gowen, Esq.; C. O. T.

Of Foreign Works, M. Tonnelier; M. Blavier; J. C. Delametherie; Hericart de Thury; H. Lelivec; C. L. Cadet; M. Guyton-Morveau.

And of British Memoirs abridged or extracted, William Herschel, L. L. D. F.R.S.; T. A. Knight, Esq. F. R S.; Mr. William Brande; Everard Home, Esq. F. R. S.; Humphry Davy, Esq. Sec. R. S. M. R. I. A.; William Henry, M. D.; Thomas Young, M. D. For. Sec. R. S.; John Goldingham, Esq. F. R. S.; William Richardson, D. D.; W. Allen, Esq. F.R.S.; W. H. Pepys, Esq. F. R. S.; Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.; Mr. James Parkinson; R. A. Salisbury, Esq. F. R. S.; Mr. John Dunbar; Henry Reeve, M. D.; Dr. James Howison; Mr. James Broad; Bath Agricultural Society; Lord Somerville; John Billingsley, Esq.; Mr. Charles le Caan; James Edward Smith, M. D. F. R. S. P. L. S.; William Hunter, Esq. Sec. A. S.; Mr. William Anderson; Dr. Alexander Anderson.

The Engravings consist of 1. An improved Goniometer, by the Rev. William Burrows: 2. Views of the late Comet, by Dr. Herschel: 3. Crystals of the Diopside, a new Species of Mineral: 4. Mr. Christopher Towill's Life Boat: 5. Delphinus Melas, a non descript of the Order Cete: 6. Dr. W. Henry's Apparatus for the Analysis of the Compound inflammable Gasses by slow Combustion: 7. Messrs. Allen and Pepys's Apparatus for showing the Changes produced in Air by Respiration: 8. View of Portmoon: 9. View of Pleskin, on the N. W. Side of Bengore Promontory: 10. Sir George Cayley's Plan for a Theatre: 11. The Chinese Method of propagating Fruit Trees: 12. Mr. Broad's Instrument for measuring standing Timber: 13. Mr. Le Caan's improved Tram Plates for Rail Roads: 14. Guyton-Morveau's permanent Apparatus for destroying Contagion.

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Mr. Burron's Goniometer

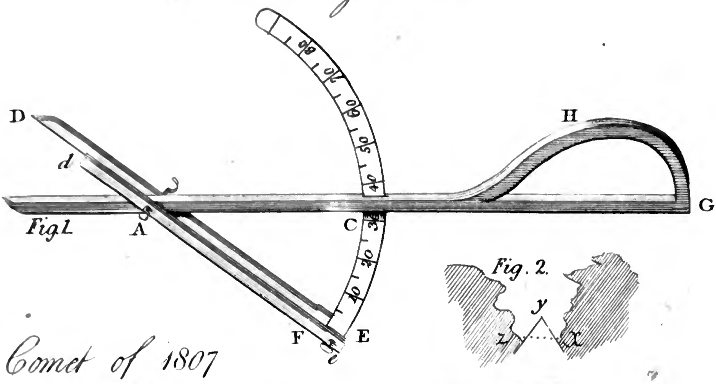
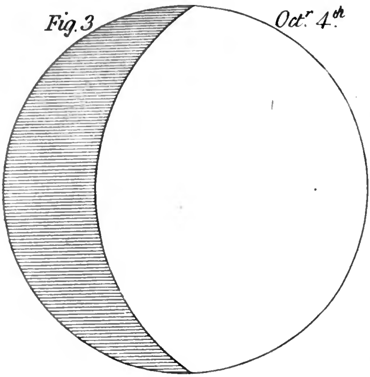


Fig. 1.

Fig. 2.

Comet of 1807

Fig. 3. Octr. 4th.



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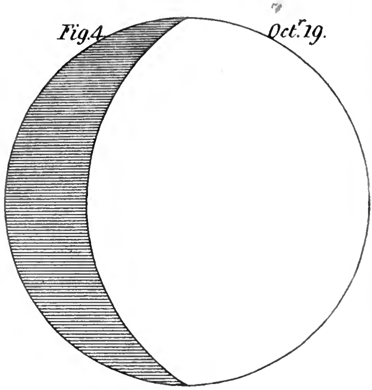
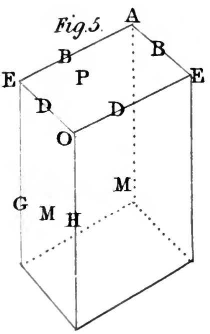
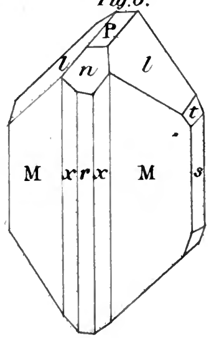


Fig. 5.



Diopside

Fig. 6.



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JANUARY, 1809.

ARTICLE I.

An account of a Goniometer invented by the Rev. E. J. BURROW, *A. M., Fellow of Magdalen College, Cambridge.*

To Mr. NICHOLSON.

SIR, *Walthamstow, Dec. 6th,* 1808.

SHOULD you think the following account, accompanied by a drawing, of a goniometer made by Mr. Harris of Holborn, under my directions, upon, I believe, a new principle, at all worthy of a place in your Journal, by inserting it you will oblige

New goniometer

Your obedient servant,

E. J. BURROW.

B G, Plate I, fig. 1, is a steel bar of about $\frac{1}{10}$ of an inch square, chamfered off to the point B. On B G is taken exactly an inch, B A, and A is made the centre of motion of the legs D E and *d e*, of which D E is also brought to a point at D to correspond with B. To the other end of these legs

described.

legs is attached by the pin F, a movable quadrant, passing through the bar B G at C, and graduated to read towards the side of the shorter leg, *d e*. The handle G H is made to project on the opposite side to that on which the legs move, that it may not interfere with the use of a brass degree divided into minutes, to be attached to the centre A upon longer radii. The leg A B is divided accurately into tenths, and the two nearest the point B into twentieths, and these again into fortieths. The whole instrument is about four inches and a half long.

Method of using it.

Now if the crystal, the angle of which is to be measured, be detached, it is obvious, that, if an acute one, by applying it to the angle B A D the vertical and equal angle E A C will be given on the quadrant: if obtuse, by applying it to the angle E and B we obtain its supplement E A C.— But if the crystal be imbedded in the matrix, so that with the common goniometers having the movable vertex it is difficult to procure a mechanical measurement of the protruding angle, we take any distance from the vertex of the crystal measured upon the scale of tenths, &c.; and placing one point of the instrument, *(ex. gr.)* on x (fig. 2) and the other on z, *so as to make the triangle* $x\,y\,z$ *isoscelles*, we get upon the quadrant the angle which the base $x\,z$ subtends at the radius one inch; and we have the side $x\,y$ measured. As, therefore, when the object is given, the angle it subtends, and $\frac{1.}{\text{rad.}}$, we have the proportion $x\,y$: A B : : angle found : angle sought. And as the whole radius is one inch divided into tenths, this proportion is easily made in the head.

Inclinations of strata ascertained by it.

The short bar is added for the better carrying of the quadrant, and renders the instrument useful in geological observations, to ascertain the inclinations of strata. This is done by hanging a small plummet to a hook at A, and having made the angle which it forms at B C a right angle, and moving the outer bars, till their upper line coincides with the stratum, the angle of inclination will be given on the quadrant.

II.

II.

Observations of a Comet, made with a view to investigate its Magnitude, and the Nature of its Illumination. By WILLIAM HERSCHEL, *LL. D. F. R. S**.

THE comet, which we have lately observed, was pointed out to me by Mr. Piggot, who discovered it at Bath the 28th of September, and the first time I had an opportunity of examining it was the 4th of October, when its brightness to the naked eye gave me great hopes to find it of a different construction from many I have seen before, in which no solid body could be discovered with any of my telescopes. Comet when seen.

In the following observations, my attention has been directed to such phenomena only, as were likely to give us some information relating to the physical condition of the comet, it will therefore not be expected, that I should give an account of its motion, which I was well assured would be most accurately ascertained at the Royal Observatory at Greenwich. Examined with respect to its physical condition.

The different parts of a comet have been generally expressed by terms that may be liable to misapprehension, such as the head, the tail, the coma, and the nucleus; for in reading what some authors say of the head, when they speak of the size of the comet, it is evident that they take it for what is often called the nucleus. The truth is, that inferior telescopes, which cannot show the real nucleus, will give a certain magnitude of the comet, which may be called its head; it includes all the very bright surrounding light; nor is the name of the head badly applied, if we keep it to this meaning; and since, with proper restriction, the terms which have been used may be retained, I shall give a short account of my observations of the comet, as they relate to the above-mentioned particulars, namely, the nucleus, the head, the coma, and the tail, without regarding the order of the time when they were made. The date of each observation, Terms relating to comets frequently misapplied.

* Philosophical Transactions for 1808, Part II, p. 145.

observation, however, will be added, that any person who may hereafter be in possession of more accurate elements of the comet's orbit, than those which I have at present, may repeat the calculations in order to obtain a more accurate result.

Of the Nucleus.

Nucleus.

From what has already been said, it will easily be understood, that, by the nucleus of the comet, I mean that part of the head which appears to be a condensed or solid body, and in which none of the very bright coma is included. It should be remarked, that from this definition it follows, that when the nucleus is very small, no telescope, but what has light and power in an eminent degree, will show it distinctly.

Observations.

Nucleus has a visible disk,

Oct. 4, 1807. 10-feet reflector. The comet has a nucleus, the disk of which is plainly to be seen.

which is real.

Oct. 6. I examined the disk of the comet with a proper set of diaphragms, such as described in a former paper*, in order to see whether any part of it were spurious; but when the exterior light was excluded, so far from appearing larger, as would have been the case with a spurious disk, it appeared rather diminished for want of light; nor was its diameter lessened when I used only the outside rays of the mirror. The visible disk of the comet therefore is a real one.

Oct. 4. I viewed the comet with different magnifying powers, but found that its light was not sufficiently intense to bear very high ones. As far as 200 and 300, my 10 feet reflector acted very well, but with 400 and 500 there was nothing gained, because the exertion of a power depending on the quantity of light was obstructed†, which I found was here of greater consequence than the increase of magnitude.

Illumination of the Nucleus.

Apparently

Oct. 4, 6h. 15′ The nucleus is apparently round, and

* See Phil. Trans. for 1805, page 53. Use of the Criterion.

† See Phil. Trans. for 1800, p. 78.

equally

equally bright all over its disk. I attended particularly to its roundness. round and uniformly bright.

Oct. 18. The nucleus is not only round, but also every where of equal brightness.

Oct. 19. I see the nucleus again, perfectly round, well defined, and equally luminous. Its brilliant colour in my 10 feet telescope is a little tinged with red; but less so than that of Arcturus to the naked eye.

Magnitude of the Nucleus.

Oct. 26. In order to see the nucleus as small as it really is, we should look at it a long while, that the eye may gradually lose the impression of the bright coma which surrounds it. This impression will diminish gradually, and when the eye has got the better of it, the nucleus will then be seen most distinctly, and of a determined magnitude. Its magnitude.

Oct. 4. With a 7 feet reflector I estimated the diameter of the nucleus of the comet at first to be about five seconds, but soon after I called it four, and by looking at it longer, I supposed it could not exceed three seconds. Diameter less than 3 seconds.

Oct. 6. 10 feet reflector, power 221. The apparent disk of the comet is much less than that of the Georgian planet, which being an object I have seen so often with the same instrument, and magnifying power, this estimation from memory cannot be very erroneous. Apparent disk less than that of Herschel.

Oct. 5. Micrometers for measuring very small diameters, when high magnifying powers cannot be used, being very little to be depended upon, I erected a set of sealing wax globules upon a post at 2422 inches from the object mirror of my 10 feet reflector, and viewed them with an eye glass, which gives the instrument a power of 221, this being the same which I had found last night to show the nucleus of the comet well. I kept them in their place all the day, and reviewed them from time to time, that their magnitudes might be more precisely remembered in the evening, when I intended to compare the appearance of the nucleus with them. Compared with globules of sealing wax.

On examining the comet, I found the diameter of its nucleus to be certainly less than the largest of my globules, which, being ·0466 of an inch, subtended an angle of 3″·97 at the distance of the telescope in the day time.

Comparing

Comparing the nucleus also with the impressions, which the view of the second and third had left in my memory, and of which the real diameters were ·0325 and ·0290 of an inch: and magnitudes at the station of the mirror 2″·77 and 2″·47, I found, that the comet was almost as large as the second, and a little larger than the third.

Oct. 18. The nucleus is less than the globule which subtends 2″·77.

Oct. 19. The air being uncommonly clear, I saw the comet 40 minutes after five, and being now at a considerable altitude, I examined it with 289, and having but very lately reviewed my globules, I judged its diameter to be not only less than my second globule, but also less than the third: that is, less than 2″·47.

Oct. 6. The 20-feet reflector, notwithstanding its great light, does not show the nucleus of the comet larger than the 10 feet, with an equal magnifier, makes it.

Oct. 28. My large 10 feet telescope, with the mirror of 24 inches in diameter, does not increase the size of the nucleus.

Compared with the 3d satellite of Jupiter.

Oct. 6. Being fully aware of the objections, that may be made against the method of comparing the magnitude of the nucleus of the comet with objects that cannot be seen together, I had recourse to the satellites of Jupiter for a more decisive result, and with my 7 feet telescope, power 202, I viewed the disk of the third satellite and of the nucleus of the comet alternately. They were both already too low to be seen very distinctly; the diameter of the nucleus however appeared to be less than twice that of the satellite.

Oct. 18. With the 10 feet reflector, and the power 221, a similar estimation was made; but the light of the moon would not permit a fair comparison.

Examined with a new mirror.

Oct. 19. I had prepared a new 10 feet mirror, the delicate polish of my former one having suffered a little from being exposed to damp air in nocturnal observations. This new one being uncommonly distinct, and the air also remarkably clear, I turned the telescope from the comet to Jupiter's third satellite, and saw its diameter very distinctly larger than the nucleus of the comet. I turned the telescope again to the comet, and as soon as I saw it distinctly round and well

well defined, I was assured that its diameter was less than that of the satellite.

6h 20′. I repeated these alternate observations, and always found the same result. The night is beautifully clear, and the moon has not yet risen to interfere with the light of the comet.

Nov. 20. With a 7 feet reflector, and power only 75, I can also see the nucleus; it is extremely small, being little more than a mere point.

Of the Head of the Comet.

When the comet is viewed with an inferior telescope, or if the magnifying power, with a pretty good one, is either much too low, or much too high, the very bright rays immediately contiguous to the nucleus will seem to belong to it, and form what may be called the head. Head of the comet.

Oct. 19. I examined the head of the comet with an indifferent telescope, in the manner I have described, and found it apparently of the size of the planet Jupiter, when it is viewed with the same telescope and magnifying power.

With a good telescope, I saw in the centre of the head a very small well defined round point.

Nov. 20. The head of the comet is now less brilliant than it has been.

Of the Coma of the Comet.

The coma is the nebulous appearance surrounding the head. Its coma.

Oct. 19. By the field of view of my reflector, I estimate the coma of the comet to be about 6 minutes in diameter.

Dec. 6. The extent of the coma, with a mirror of 24 inches diameter, is now about 4′ 45″.

Of the Tail of the Comet.

Oct, 18. 7h. With a night glass, which has a field of view of nearly 5°, I estimated the length of the tail to be $3^{\circ}\frac{3}{4}$; but twilight is still very strong, which may prevent my seeing the whole of it. Its tail.

Nov. 20. The tail of the comet is still of a considerable length, certainly not less than $2\frac{1}{2}$ degrees.

Oct.

Oct. 26. The tail of the comet is considerably longer on the south-preceding, than on the north-following side.

It is not bifid, as I have seen the comet of 1769 delineated by a gentleman who had carefully observed it*.

Oct. 28. 7 feet reflector. The south-preceding side of the tail in all its length, except towards the end, is very well defined; but the north-following side is every where hazy and irregular, especially towards the end; it is also shorter than the south-preceding one.

The shape of the unequal length of the sides of the tail, when attentively viewed, is visible in a night glass, and even to the naked eye.

Oct. 31. 10 feet reflector. The tail continues to be better defined on the south-preceding than on the north-following side.

Dec. 6. The length of the tail is now reduced to about 23′ of a degree.

Of the Density of the Coma and Tail of the Comet.

Density of the coma and tail

Many authors have said, that the tails of comets are of so rare a texture, as not to affect the light of the smallest stars that are seen through them. Unwilling to take any thing upon trust, that may he brought to the test of observation, I took notice of many small stars, that were occasionally covered by the coma and the tail, and the result is as follows.

sufficient to obscure the stars seen through them

Oct. 26, 6h. 15′. Large 10 feet reflector, 24 inches aperture. A small star within the coma is equally faint with two other stars that are on the north-following side of the comet, but without the coma.

7h. 30′. The coma being partly removed from the star, it is now brighter than it was before.

Oct. 31, 6h. 5′. 10 feet reflector. A star in the tail of the comet, which we will call *a*, is much less bright than two others, *b* and *c*, without the tail.

Two other stars, *d* and *e*, towards the south of *b* and *c*, are in the following skirts of the tail, and are extremely faint.

7h. 20′. The star *e* is now considerably bright, the tail

* Dr. Lind of Windsor.

having

having left it, while *d*, which is rather more involved than it was before, is hardly to be seen.

7h. 50′. The star *a*, toward which the comet moves, is involved in denser nebulosity than before, and is grown fainter.

d is involved in brighter nebulosity than before, but being near the margin, it will soon emerge.

8h. 35′. Being still more involved, the star *a* is now hardly visible.

e is quite clear of the tail, and is a considerable star; *d* remains involved.

9h. 10′. The star *d* is also emerged, but the comet is now too low to estimate the brightness of stars properly.

Nov. 25, 7h. 35′. There is a star *a* within the light of the tail, near the head of the comet, equal to a star *b* situate without the tail, but near enough to be seen in the field of view with *a*. The path of the head of the comet leads towards *a*, and a more intense brightness will come upon it.

8h. 46′. The star *a* is now involved in the brightness near the head of the comet, and is no longer visible, except now and then very faintly, by occasional imperfect glimpses; but the star *b* retains its former light.

Nebulous appearance of the Comet.

Dec. 6. The head of the comet, viewed with a mirror of 24 inches diameter, resembles now one of those nebulæ, which in my catalogues would have been described, " a very large, brilliant, round nebula, suddenly much brighter in the middle." Nebulous appearance of the comet.

Dec. 16. 7 feet reflector. The night being fine, and the moon not risen, the comet resembles " a very bright, large, irregular, round nebula, very gradually much brighter in the middle, with a faint nebulosity on the south-preceding side."

Jan. 1, 1808. 7 feet. " Very bright, very large, very gradually much brighter in the middle."

If I had not known this to be a comet, I should have added to my description of it as a nebula, that the centre of it might consist of very small stars; but this being impossible, I directed my 10 feet telescope with a high power to the comet

met, in order to ascertain the cause of this appearance; in consequence of which I perceived several small stars shining through the nebulosity of the coma.

Jan. 14. 7 feet. " Bright, pretty large, irregular round, brighter in the middle."

Feb. 2. 10 feet, 24 inch aperture. " Very bright, large, irregular round, very gradually much brighter in the middle." There is a very faint diffused nebulosity on the north preceding side; I take it to be the vanishing remains of the comet's tail.

Feb. 19. Considerably bright; about $\frac{1}{7}$ of the field $=$ 3′ 26″ " in diameter, gradually brighter in the middle." The faint nebulosity in the place where the tail used to be still projects a little farther from the centre than in other directions.

Feb. 21. Less bright than on the 19th; nearly of the same size: gradually brighter in the middle. The nebulosity still a little projecting on the side where the tail used to be.

Result of the foregoing Observations.

General inferences.

From the observations which are now before us, we may draw some inferences, which will be of considerable importance with regard to the information they give us, not only of the size of the comet, but also of the nature of its illumination.

It is a solid body,

A visible, round, and well defined disk, shining in every part of it with equal brightness, elucidates two material circumstances; for since the nucleus of this comet, like the body of a planet, appeared in the shape of a disk, which was experimentally found to be a real one, we have good reason to believe, that it consists of some condensed or solid body, the magnitude of which may be ascertained by calculation. For instance, we have seen, that its apparent diameter, the 19th of October, 6h. 20′, was not quite so large as that of the third satellite of Jupiter. In order therefore to have some idea of the real magnitude of our comet, we may admit, that its diameter at the time of observation was about 1″, which certainly cannot be far from truth. The diameter of the 3d satellite of Jupiter, however, is known to have a permanent disk, such as may at any convenient time be measured

measured with all the accuracy that can be used; and when the result of such a measure has given us the diameter of this satellite, it may by calculation be brought to the distance from the Earth at which, in my observation, it was compared with the diameter of the comet, and thus more accuracy, if it should be required, may be obtained. The following result of my calculation however appears to me quite sufficient for the purpose of general information. From the perihelion distance 0·647491, and the rest of the given elements of the comet, we find, that its distance from the ascending node on its orbit at the time of observation was 73° 45′ 44″; and having also the Earth's distance from the same node, and the inclination of the comet's orbit, we compute by these data the angle at the sun. Then by calculating in the next place the radius vector of the comet, and having likewise the distance of the Earth from the sun, we find by computation, that the distance of the comet from the Earth at the time of observation was 1·169192, the mean distance of the Earth being 1. Now since the disk of the comet was observed to subtend an angle of 1″, which brought to the mean distance of the Earth gives 1″·169, and since we also know that the Earth's diameter, which, according to Mr. Dalby, is 7913·2 miles*, subtends at the same distance an angle of 17″·2 we deduce from these principles the real diameter of the comet, which is 538 miles. 538 miles in diameter.

Having thus investigated the magnitude of our comet, we may in the next place also apply calculation to its illumination. The observations relating to the light of the comet were made from the 4th of October to the 19th. In all which time the comet uniformly preserved the appearance of a planetary disk fully enlightened by the sun: it was every where equally bright, round, and well defined on its borders. Now as that part of the disk which was then visible to us could not possibly have a full illumination from the sun, I have calculated the phases of the comet for the Its illumination.

* See Phil. Trans. for 1791, page 239. Mr. Dalby gives the two semiaxes of the Earth, from a mean of which the above diameter 7913·1682 is obtained.

4th

4th and for the 19th, the result of which is, that on the 4th the illumination was 119° 45′ 9″, as represented in Pl. I, fig. 3, and that on the 19th it had gradually increased to 124° 22′ 40″, of which a representation is given in fig. 4. Both phases appear to me sufficiently defalcated, to prove that the comet did not shine by light reflected from the sun only; for had this been the case, the deficiency I think would have been perceived, notwithstanding the smallness of the object. Those who are acquainted with my experiments on small silver globules* will easily admit, that the same telescope, which could show the spherical form of balls, that subtended only a few tenths of a second in diameter, would surely not have represented a cometary disk as circular, if it had been as deficient as are the figures which give the calculated appearances.

indicates it to be luminous of itself.

If these remarks are well founded, we are authorised to conclude, that the body of the comet on its surface is self-luminous, from whatever cause this quality may be derived. The vivacity of the light of the comet also had a much greater resemblance to the radiance of the stars, than to the mild reflection of the sun's beams from the moon, which is an additional support of our former inference.

Which is farther proved by the stars seen through the tail.

The changes in the brightness of the small stars, when they are successively immerged in the tail or coma of the comet, or cleared from them, prove evidently, that they are sufficiently dense to obstruct the free passage of star-light. Indeed if the tail or coma were composed of particles that reflect the light of the sun, to make them visible we ought rather to expect, that the number of solid reflecting particles, required for this purpose, would entirely prevent our seeing any stars through them. But the brightness of the head, coma, and tail alone, will sufficiently account for the observed changes, if we admit, that they shine not by reflection, but by their own radiance; for a faint object projected on a bright ground, or seen through it, will certainly appear somewhat fainter, although its rays should meet with no obstruction in coming to the eye. Now, as in this case we are sure of the bright interposition of the parts of the comet,

* See Phil. Trans. for 1805, p. 38, the 5th experiment.

but

but have no knowledge of floating particles, we ought certainly not to ascribe an effect to a hypothetical cause, when the existence of one, quite sufficient to explain the phenomena, is evident.

Other circumstances prove the same.

If we admit, that the observed full illumination of the disk of the comet cannot be accounted for from reflection, we may draw the same conclusion, with respect to the brightness of the head, coma, and tail, from the following consideration. The observation of the 2d of February mentions, that not only the head and coma were still very bright, but that also the faint remains of the tail were still visible; but the distance of the comet from the Earth, at the time of observation, was nearly 240 millions of miles*, which proves, I think, that no light reflected from floating particles could possibly have reached the eye, without supposing the number, extent, and density of these particles far greater than what can be admitted.

My last observation of the comet, on the 21st of February, gives additional support to what has been said; for at the time of this observation, the comet was almost 2·9 times the mean distance of the sun from the Earth†. It was also nearly 2·7 from the sun‡. What chance then could rays going to the comet from the sun, at such a distance, have to be seen after reflection, by an eye placed at more than 275 millions of miles § from the comet? And yet the instant the comet made its appearance in the telescope, it struck the eye as a very conspicuous object.

Tails of comets not vapour.

The immense tails also of some comets that have been observed, and even that of the present one, the tail of which, on the 18th of October, was expanded over a space of more than 9 millions of miles||, may be accounted for more satisfactorily, by admitting them to consist of radiant matter, such as, for instance, the aurora borealis, than when we unnecessarily ascribe their light to a reflection of the sun's il-

* 239894939.

† The sun's mean distance being 1, that of the comet was 2·89797.

‡ The comet's distance from the sun was 2·683196.

§ 275077889.

|| 9160542.

lumination

lumination thrown upon vapours supposed to arise from the body of the comet.

By the gradual increase of the distance of our comet, we have seen, that it assumed the resemblance of a nebula; and it is certain, that had I met with it in one of my sweeps of the zones of the heavens, as it appeared on either of the days between the 6th of December, and the 21st of February, it would have been put down in the list I have given of nebulæ. This remark cannot but raise a suspicion, that some comets may have actually been seen under a nebulous form, and as such have been recorded in my catalogues; and were it not a task of many years labour, I should undertake a review of all my nebulæ, in order to see whether any of them were wanting, or had changed their place, which certainly would be an investigation, that might lead to very interesting conclusions.

III.

Remarks on the Diopside, a new Species in Mineralogy established by Mr. Haüy, comprising two Varieties found in the Piedmontese Alps by Mr. Bonvoisin, *and described in the Journal de Physique for May,* 1806, *under the names of Mussite and Alalite. By Mr.* Tonnelier, *Keeper of the Cabinet of Mineralogy to the Council of Mines*.*

Discovery of new substances the best reward of the naturalist.

THE naturalist, who is led by his zeal to researches attended with much toil, feels himself well rewarded, if in his travels he be so fortunate, as to meet with substances not yet known. Such discoveries he considers as the most valuable recompense of his labours; and he deems it his duty, to publish descriptions of the new objects, with which he has enriched the field of science. Mr. Bonvoisin, a much respected natural philosopher, member of the imperial academy of Turin and of the legislative body, has recently experienced this satisfaction. Many celebrated naturalists had visited the Piedmontese Alps before him, and made known to us, among the subjects they have had opportunities of ob-

This occurred to Bonvoisin in the Piedmontese Alps.

* Journal des Mines, vol. XX, p. 65.

serving,

serving, those that appeared to them most interesting either for their novelty or their rarity. The lithology of these regions, less assiduously cultivated than the other branches of natural history, appeared to the learned academician an ample field, in which science might promise itself a rich harvest. His expectations were not disappointed, and the result of his researches, the merit of which is enhanced by the difficulties he had to overcome, is an account of his travels, the speedy appearance of which he gives us reason to hope. In the mean time we have to express our thanks to Mr. Bonvoisin for having made us acquainted with the principal substances he has collected in his mineralogical tour. Of these a very ample description is given in the Journal de Physique for May 1806, but my attention is confined at present to two of them, to which he has given the provisional names of mussite and alalite.

Two of the new substances found by him particularly noticed.

A few days ago I was present at a meeting, at which Mr. Haüy exhibited to several of his pupils the new substances, which he purposes to describe in his course of lectures this year, and among others those which Mr. Bonvoisin has sent from Piedmont to Mr. Fourcroy, who has destined the most remarkable for the gallery of the Museum of Natural History*. Among these substances two particularly engaged my attention, which the celebrated professor of mineralogy informed us he had been led by his observations to unite in one species, the essential characters of which differ completely from those that distinguish all the known species, notwithstanding their appearance seems to indicate, that they should be separated. The constant occupation, which the approaching publication of the second edition of his Treatise on Natural Philosophy imposes on Mr. Haüy, not allowing him to publish the results of his examination of the substances in question†, I requested his permission, to take

* Mr. Haüy has since given a description of the diopside, which constitutes the subject of the present paper, in the public lecture on mineralogy, which he gave at rhe Museum of Natural History on the 12th of July this year.

† With respect to the diopside Mr. Haüy has followed his usual custom of committing to writing the new observations he delivers to his pupils, and depositing a copy in the library of the Museum, which every one is at liberty to transcribe after the lecture.

this

this task upon myself, and drew up an article on the subject for the Journal des Mines, persuaded, that these new observations could not fail to be interesting to its readers. This I now purpose to perform, tracing here the principal characters of the two substances discovered by Mr. Bonvoisin.

Characters of the diopside.

Characters of the new species diopside.

Its specific gravity is 3·2374. It does not scratch glass, or very slightly; but scratches fluate of lime. Before the blowpipe it fuses into a glass of the same greenish colour as the mass itself. Its primitive form is a right angled quadrangular prism, Pl. I, fig. 5, with oblique bases, the angle of incidence between the diagonal drawn from A to O and the edge H is 107° 8′. The prism is subdivisible by very clean sections in the direction of the diagonals of its bases‡. The divisions parallel to the bases are in general very clean; those that answer to the sides *m m* are less easy to obtain.

I. *Determinate varieties of form.*

Determinate varieties of form.

The two principal varieties pointed out by Mr. Haüy are,

Primitive.

1st variety, Primitive diopside: a variety of the mussite of Mr. Bonvoisin. Fig. 5.

Care must be taken, not to confound the joints exposed on breaking the crystals exhibiting this variety with its natural bases.

Didodecaedral

2d variety, Didodecaedral diopside, fig. 6. A twelve-sided prism, terminated at each extremity by six faces, situate two and two, one above the other.

The angle of incidence between *M* and *M* is	90°
r and *M*	135°
s and *M*	135°
p and *r*	107° 8′
n and *r*	137° 12′
l and *s*	145° 53′
l and *p*	124° 7′
t and *s*	161° 16′
x and *r*	153° 26′
l and *M*	134° 36′
l and the surface contiguous to *S*, behind the crystal,	117° 55′

This

‡ If from the point O a perpendicular be let fall on the side opposite to the edge H, the ratio between this perpendicular and the part intercepted

This figure is analogous to that described in the Journal de Physique, May 1806, p. 430, as belonging to the regular crystals of alalite. The author of the description was aware, that, to give an accurate idea of a crystal, the number and position of the faces will not be sufficient, but that the quantities of the angles they form with each other must be assigned; so that if he had contented himself with mentioning the number of sides that form the prism; and the faces that terminate it, he would have given but a very imperfect idea of the crystalline form he wished to make known, as the description would apply to several different figures, on which account it would be of little use. However, having had recourse to the goniometer alone, the measures of which are merely approximations, without the assistance of calculation, he has not sufficiently guarded against the danger of giving angles inconsistent with the principles of geometry, to which the crystallographer exposes himself, who neglects the resources of algebra. Thus the faces M M being acknowledged to form a right angle with each other, and the angles of incidence between *l* M, and *l s*, or the edge that sometimes occupies the place of the latter, being given, the third, or inclination of the face adjacent to *s*, follows necessarily. Now on calculating this from the other data in the Journal de Physique, we find, that the result of the mechanical measurement is erroneous by several degrees.

The angles of a crystal must be accurately ascertained,

which cannot be done by measurement merely.

The descriptions of Romé-de-l'Isle, though in general accurate, sometimes exhibit instances of these contradictions between the values of the angles of the same crystal. This learned gentleman, for instance, after having given 105° as the great angle of the rhombus of inverted carbonate of lime, which he called muriatic calcareous spar, gives 115° for the great angle of the principle section, or that which passes through the oblique diagonals of two opposite faces, and the intermediate edges*. Now if we take the first angle as given, we shall be led by calculation to 109°4' for the second, which makes a difference of more than 5° degrees

Romé de l'Isle sometimes mistaken from trusting to the goniometer.

cepted by it toward the point A will be $\sqrt{21} : \sqrt{2}$; and this intercepted part will be to either of the edges, G or H, as 1 to 5.

* *Cristallographie*, vol. I, p. 520.

from

from the angle determined by observation. This value is too far from the truth, not to lead us to a suspicion of some irregularities in the crystal, which that celebrated crystallographer examined. It is equally certain, that calculation would have acquainted him with his mistake, in affording him a sure method of correcting his observation. I might mention other instances, if I were not afraid of wandering from my subject. That which I have adduced is sufficient, to show the justice of the remarks on the manner of describing crystals may by Mr. Haüy in the Annals of the Museum of Natural History. This gentleman has there shown, that the descriptions of a crystal, to be precise, must indicate the angles as determined by the concurrence of common geometry with that founded on the structure of minerals. By following this method, the only one compatible with strict accuracy, we shall be certain, that the angles will always agree with each other. They will be as so many limits, to which the observer may come sufficiently near with the goniometer, to be able to refer a crystal to the species or variety, the characters of which it bears. This is all we can expect from this instrument, however nicely it may be executed, and however skilful the hand that employs it.

Geometrical calculation therefore necessary

assisted by the laws of crystallization.

The diopside a distinct species.

To return to the diopside. Though this substance has never yet been subjected to analysis, Mr. Haüy does not hesitate to consider it as a species, that should occupy a distinct place in the system*. The few characters I have given are sufficient, to demonstrate this assertion, because they occupy the first rank among those that are truly specific. The primitive form obtained by mechanical division differs from all other known forms. Far from having the character of a limitation, it is remarkable for a singularity, that no other species has yet presented. This consists in the double appearance it exhibits, one in its prism, which is a foursided

The diopside has some analogy to the pyroxene.

* The place assigned to the diopside is immediately after the pyroxene, the primitive form of which bears some analogy to it. In each the primitive form is a quadrangular prism: but Mr. Haüy has ascertained, that this prism is rectangular in the diopside, while the faces of that of the pyroxene are inclined to each other in angles of about 92° and 88°. Besides, the primitive form of the diopside is subdivisible in the direction of the two diagonals of its bases, while that of the pyroxene is capable of being subdivided only parallel to the greater diagonal.

rectangle,

rectangle, the other in its bases, which are rhombs inclined to the sides of the prism. From this circumstance Mr. Haüy has suggested the name of diopside (double face.)

II. *Indeterminate varieties of form.*

Indeterminate varieties.

3. Compressed and laminiform diopside*. While Mr. Haüy was examining the crystalline forms of the new species, Mr. Tondi, a mineralogist of distinguished merit belonging to the Museum of Natural History, looking over the collection Mr. Bonvoisin had sent, which was accompanied by a systematic catalogue, observed the flattened variety among some specimens of a different species. This variety, which belongs to the mussite, afforded Mr. Haüy the mechanical division, by which the species is characterized. Compressed & laminiform.

4. Cylindroid diopside: in prisms full of grooves or striæ. Cylindroid.

5. Compact diopside. If we examine attentively the crystals of mussite, we see them prolonged in an uninterrupted series into a compact mass, which serves as their gangue, is of the same colour, though often not so dark, and cannot fail to be perceived to be the same substance, though in a less perfect state of crystallization, as Mr. Bonvoisin conjectured. Compact.

The colours of the diopside are green, greenish gray, greenish white, and yellowish white. It is sometimes translucid, sometimes opake. Colours.

The crystals of mussite are small, elongated, and commonly opake. Several are twisted, and exhibit the primitive form very undecidedly. The crystals of alalite are in general larger, translucid, and of a greenish white. Crystals.

The mussite has been found in the commune of la Balme-de-Mussa, in the department of the Po, toward the north of the valley of Lans, in the interstices of a vein one or two yards thick, that traverses, at the height of four or five yards, a rock called the Black Rock, which is twelve or fifteen yards high. The crystals have sometimes a translucid granular carbonate of lime for their gangue. Mussite where found.

* This variety of form answers to what Werner calls *strahliger*, radiated.

Alalite where found.

The alalite has been discovered in a vein in the mountain of Ciarmetta, situate beyond that of Testa-Ciarva, at the Alp of la Mussa, near the village of Ala. It is commonly accompanied with green or pale yellow primitive garnets, and emarginated garnets of a hyacinth red, which have nothing in common with the topaz. It was these last that Mr. Bonvoisin designated by the name of topazolites, because he thought them of a pleasing colour, which he compared with the yellow of the topaz.

The pyrophysalite a topaz.

I shall avail myself of this opportunity to announce, that Mr. Haüy has found the substance called pyrophysalite by Messrs. Hisinger and Berzelius, an analysis of which was given by these gentlemen in the Annales de Chimie for May, 1806*, to be a variety of the aluminous fluate of silex (topaz), of a greenish white colour, and nearly opake.

IV.

Letter from Sir THOMAS CLARGES, *Bart. of Sutton upon Derwent, to* W. ANNESLEY, *Esq., on the Subject of Life-boats.*

DEAR SIR, *Portsmouth, Nov.* 29, 1808.

Lukin's patent boat.

I Promised to give you an account of Life-boats. In the first place there is one by Lukin, late a coach-maker in Long-Acre, who has published a treatise on it. It is a row-boat, and on the sides has something similar to mine, a hollow case made air-tight, running the whole length of the boat. A bird must have wings on each side to support it in the air; and thus we technically give the name of wings to those parts on the sides of a boat, that tend to give it buoyancy. On the outside of the airtight cases in Lukin's boat is a layer or belt of cork: so that his wings are formed of airtight cases, to buoy up the boat in the water like bladders, and of cork. To prevent the boat from upsetting, it is furnished with a cast iron keel.

Buoyed up by cork, and air-tight cases.

Balanced by an iron keel.

Objections to it.

The objections to Mr. Lukin's boat are these. The air-tight cases are not in compartments, or chambers, like mine; and therefore, if forced through by a rock, or striking against

* See our Journal, vol. XIX, p. 33.

any

Mr. Christopher Towill's Life Boat.

Fig. 1.

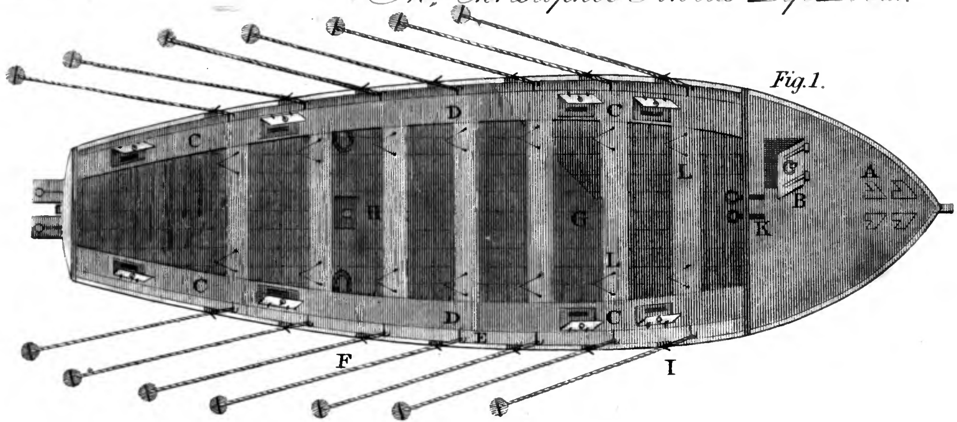


Fig. 2.

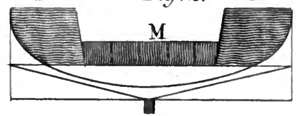


Fig. 3

Fig. 5.

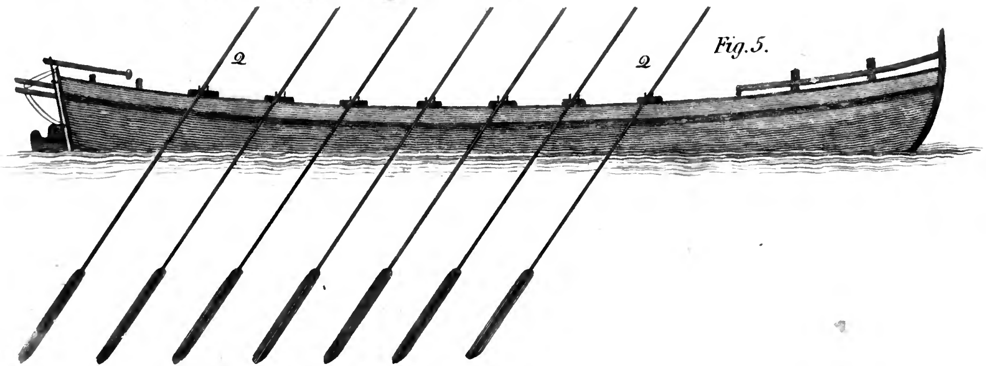
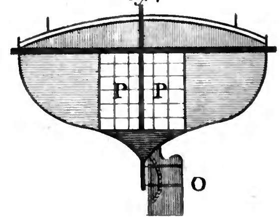


Fig. 4



any projecting part of the wreck, the whole length would fill with water. Besides, they do not open at top like mine, nor, if they did, are they large enough to afford cabin room. Cork, if used in sufficient quantity to be serviceable, occupies considerable space, and likewise affords no room. The iron keel, though safe for sailing, is a very great incumbrance in rowing, particularly against a high wind and heavy sea, so that the boat could not move very fast. My boat, being large and wide, could never be upset, unless the men did it purposely; yet would answer briskly and steadily to the oars.

Shields, or Greathead's boat.

I now come to the Shields boat, improperly called Greathead's. This is sharp at both ends, like the funny-boats about Westminster bridge, to row with either end foremost. The wings are of cork, of which there are two layers, or belts, on each side, one without the gunwale, the other within. The history of the Shield's boat in brief is this. Some gentlemen of Shields, seeing the frequent occurrence of the dreadful accident of shipwreck, offered a premium of two guineas to the person, who should produce the best model of a life-boat; and a committee was appointed, to examine and decide on the models.

Wouldhave's invention.

A painter of Newcastle, of the name of Wouldhave, made a model of copper, sharp and high at both ends, and belted with cork. This, being tried in a tub of water, would not sink when full; and when upset, it righted again.

Greathead's.

Mr. Greathead, a shipbuilder of Shields, gave in a model of a boat he had seen in America, in shape much resembling a butcher's tray. This had a singular property for a life boat, it soon upset, and they could not get it back again, but it whold keep its bottom uppermost,

The committee however rejected Wouldhave's, because copper would not swim; and gave the prize to Greathead, because his model was made of wood. Two gentlemen of the committee went into a brickmaker's, moulded a model in clay, nearly the same shape as Wouldhave's, and floated with cork like his, and gave orders to Greathead to build a boat from this model. Greathead suggested a curved keel instead of a straight one, which was adopted. Thus his claim

claim to invention was building boats of wood, and making them like rocking boats.

Objections to this boat.

The objections to this boat are glaring. The rowing against a heavy sea and wind with such elevated ends is like rowing against the wind with sails set. The idea of the gentlemen was to keep out the water. But what signified the water coming in, if the boat would not sink? Besides, a boat thus formed would be filled amidships by the first wave, in spite of the high ends. Then as the oars lie on a curved gunwale, the poor rowers at each end will be scarcely able to dip their blades in the water; while those in the middle will be constantly catching crabs, as the sailors call it, and getting severe blows on the stomach. The curved keel too has the great inconvenience of making the boat rest as it were on a point, so as to be liable every moment to be turned round, and laid athwart the waves: and if she be not turned round, when a heavy wave strikes against her high bow, from being sharp at both ends she will run backward much faster than the rowers can pull her forward. Now mine is intended only to row forward, and her broad stern hinders her from being driven backward. There is one advantage however in rowing either end foremost; as the boat is liable from her curved keel to be spun round every minute, the men need not concern themselves about getting her round again, but may row on stern foremost, as if nothing had happened. In fact the Shields boat is a farce to a real seaman, and never attempts any thing but moderate seas*. It is indeed much worse constructed for the purpose than any common row-boat, in all respects except the addition of cork; and this even Wouldhave did not invent in the eye of the law, since Lukin had a patent for it years before. Wouldhave's idea of high ends, to make the boat right herself again, if she upset, though not so bad as that of keeping the water out, is like the proverb of penny-wise and pound-foolish: for you cannot well upset a large wide boat in rowing, and such ends may defeat the chief object, that of being able to put out to a wreck against a tremendous wind and sea, which is often to be expected. Another fault

* See our Journal, vol. XXI, p. 132: and for a description of a life-boat by Mr. Wilson, p. 124.

of

of a curved keel is the difficulty it occasions of steering the boat straight, particularly when from the height of the end the steersman is so high above the water.

Description of a new life boat.

To come to my boat, of which a plan is given in plate 2. You will perceive from her size and width, that she is secure from upsetting, without being incumbered below with an iron keel, or above with high ends. She cannot sink, which is the chief thing. Her wings are close; and yet they can be opened to admit many things. The space of the boat is not occupied by belts of cork, or hollow cases that cannot open, like Lukin's: while the wings, from their size, afford cabin room; and, being in compartments, if a rock force through one, the others remaining sound will prevent the water from rising much in that which is damaged. Perhaps Mr. Lukin would say, if these be opened, and any thing put in them, they will cease to be wings, and will not buoy up the boat like a hollow case: but you know one great secret of buoyancy is the resistance at bottom. Thus put an empty basin into water, and try to sink it, and you will find how great its resistance is, though completely open at top: or by putting weights into it you may satisfy yourself, that it will float a considerable burden. In mine the burden can be suited to the occasion; and if not wanted to hold any thing, keep the lids close, and they are airtight buoys. As the wings of my boat are so broad, they form a deck, on which people may lie, particularly if a few iron stanchions be placed along the gunwale and connected by ropes. These stanchions might have a joint at the bottom, allowing them to yield inwardly, but not outwardly, which would prevent any damage if a heavy sea should roll the boat against the wreck.

Her peculiarities. A large cabin,

You must remember a peculiar feature in the boat, the large cabin in the bow for women and children, with ventilators on the valve principle, to admit air upwards, but no water downwards. I need not dwell on the importance of this cabin, in cases of shipwreck, to affrighted and half drowned women and children. It is obviously an indispensable requisite to a life-boat, that it should be incapable of sinking; but mine cannot even fill, or be water-logged. This is effected by very simple means, which do not appear even

and after ports to discharge the water.

even to have been thought of before. It must be observed, that the little space in the midships, where the rowers sit, is the only part in which the water, that breaks in, can lodge, But as the bottom of this space is above the level of the water without, there are two portholes at the stern, opening outwardly like valves, at which the water will discharge itself; and when they fall too, the heaviest sea striking the stern, instead of finding its way in, will only serve to close them.

Her good propertics.

Having stated the leading features of my boat, namely, that she will not upset, or sink, or be waterlogged; that she affords cabin room; that she is like a man of war's launch, well built for rowing, the oars not on a curve, but nearly in a right line, and low to the water, of which she draws little, a circumstance that renders her a fine pilot boat; I need not dwell on a few secondary points, which however it would be improper not to mention. These are her being provided with small ropes, or lines, fastened to hooks on the gunwale, and each having a piece of cork painted red at the extremity; intended not only for persons who fall overboard, or swim from a wreck, to see and catch hold of; but to tow any for whom there may not be room in the boat: and her having a very powerful rudder, which reaches some inches below the keel, but will haul up level with it, when going in very shallow water, and then let down again. The copper cases, though affording additional security to those who choose to be at the expense, are no more a necessary point of my plan, than coppering her bottom. The woodwork alone, if well executed and properly attended to, may be kept quite airtight. If the assistance of cork were to be called in, it appears to me, that it might be better applied than in the other boats, by filling the cases with cork jackets, to take to a crowded wreck; in going off to which the cases would not be wanted for any other purpose, and the jackets would not be an encumbrance. You must be aware of the importance of the side cabins, or cases, for stowing valuable goods from a richly laden vessel.

Lines floated by cork.

Peculiar rudder.

Copper cases not essential.

Useful to ships on discovery, or any large ships.

A boat of this kind, but somewhat smaller dimensions, would be exceedingly useful to ships on voyages of discovery, and indeed to any large vessels; as it would not only answer for wooding and watering, but is peculiarly adapted

for

for excursions up rivers, or small inlets of the sea, or exploring clusters of islands.

As a pleasure boat she answers extremely well. And with respect to her safety I can say, that I have sailed in her from Brighton round the Cornish coast to Conway in North Wales, without any accident, though we experienced some very dreadful weather on the voyage. Excellent pleasure boat.

The following is a description of the boat, as built by Mr. Christopher Towill, of Teignmouth, see Plate 2. Description of this boat.

Her length is 30 feet, her breadth 10, her depth 3 feet 6 inches. The space between her timbers is fitted up with pine wood; this is done with a view to prevent the water lodging there. The pine wood is well caulked and paid.

She is buoyed up by 8 metal cases, 4 on each side; these are watertight, and independent of each other. They will serve to buoy up six tons; but I find that all the buoyant parts of the boat, taken collectively, will buoy up ten tons. The cases are securely decked over, and boarded at the sides with pine. There is a scuttle to each case to put goods in; the edges are lined with baize; and over each scuttle, in the case, is one of wood, of a larger size, the margin of which is lined in the same manner to exclude the water. Between the cases are Norwegian balks, bolted to the bottom, fastened to each other by iron clamps, and decked over. The depth of her keel is about 9 inches below the garboard-streak, the dead rising is 4 inches. Her keel is narrow at the under part, and wide above, for the purpose of giving the timber a good bed, which will support the bolts, in case a necessity should arise to encounter sandbanks.

In sailing over a bar, or in places where the water is shallow, the rudder will, with ease, draw up even with the keel, and when in deep water, it will let down, instantly, and with equal facility, a foot below it; in consequence of which advantage, the boat is found to steer remarkably well.

The forecastle of the boat forms a cabin 10 feet wide, 6 feet long, and 4 feet deep, into which women, children, and disabled persons may be put. It is amply supplied with air, by means of two copper ventilators. It is furnished besides with two grapnels, very proper to be thrown on board a wreck, to ride by. The grapnel ropes will assist the sufferers

ferers to remove, and escape from the wreck to the boat. She is likewise equipped with masts and sails, and is as manageable with them as any boat of her dimensions can possibly be.

In a tempest, however, she must be dismasted, and rowed by fourteen men, with oars 16 feet long, double banked. The men are all fastened to the thwarts by ropes, and cannot be washed from their seats,

Explanation of the plate.

Explanation of the plate.

Fig. 1. A, Four copper ventilators.

B. The forecastle skuttle.

C, C, C, C. Four wood scuttles on each side.

D, D. The deck that covers the copper cases, wherein seven tons of goods may be put, or other articles,

E. Hooks for the life lines,

F. Seven life-lines on each side, the ends in the water, floated with cork, by which men may hold that are washed from the wreck before they can be taken into the boat,

G. The grating in the bottom of the boat,

H. Three small pump wells.

I. The tholes.

K. The bitts.

L, L. Ropes to fasten the men to the thwarts,

Fig. 2. Section of her midship part.

M. Five Norway baulks.

N, N. The copper cases.

Fig. 3. A perspective view of one of the cases detached.

Fig. 4. The stern.

O. The rudder on a new principle.

P, P. The stern ports to let out the water.

Fig. 5. A side view of the boat, to show her sheer.

Q, Q. The oars.

V.

On the Origin and Office of the Alburnum of Trees. In a Letter from T. A. Knight, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. K. B. P. R. S**.

My Dear Sir,

In my last communication I endeavoured to prove, that the bark of trees is not subsequently transmuted into alburnum; and if the statements that I have there given be correct, they are, I conceive, decisive on the point for which I contended: and if the bark be not converted into alburnum, the experiments of Duhamel, and subsequent naturalists, and those of which I have given an account in former memoirs, afford sufficient evidence, that the bark deposits the alburnous matter. If the succulent shoot of a horse chesnut, or other tree, be examined, at successive periods in the spring, it will be seen, that the alburnum is deposited, and its tubes arranged, in ridges beneath the cortical vessels; and the number of these ridges, at the base of each leaf, will be found to correspond accurately with the number of apertures through which the vessels pass from the leaf-stalks into the interior bark, the alburnous matter being apparently deposited (as I have endeavoured to prove in former memoirs) by a fluid which descends from the leaves, and subsequently secretes through the bark†. I shall therefore venture to conclude, that it is thus deposited, and shall proceed to enquire into the origin and office of the alburnous tubes.

The bark not changed into alburnum

but deposits the alburnous matter.

The position and direction of these tubes have induced almost all naturalists to consider them as the passages through which the sap ascends; and at their first formation, when the substance which surrounds them is still soft and succulent, they are always filled with the fluid, which has apparently secreted from the bark. They appear to be formed in the soft cellular mass, which becomes the future alburnum,

Origin and office of the alburnous tubes.

* Philos. Trans. for 1808, Part II, p. 313.

† Phil. Trans. 1807, p. 336.

num, as receptacles of this fluid, to which they may either afford a passage upwards, or simply retain it as reservoirs, till absorbed, and carried off, by the surrounding cellular substance. The former supposition is, at first view, the most probable; but the latter is much more consistent with the circumstances, that I shall proceed to state.

Ascent of the sap.

Many different hypotheses have been offered by naturalists to account for the force, with which the sap ascends in the spring; of these hypotheses two only appear in any degree adequate to the effects produced. Saussure jun. supposes, that the tubes contract as soon as they have received the sap in the root, and that this contraction, commencing in the root, proceeds upwards, impelling the sap before it: and I have suggested, that the expansion and contraction of the compressed cellular, or laminated substance (the tissu cellulaire of Duhamel and Mirbel) which expands and contracts with change of temperature* after the tree has ceased to live, might produce similar effects, by occasioning nearly a similar motion and compression of the tubes, the coats of which are, I believe, universally admitted not to be membranous. But both these hypotheses are inconsistent with the facts, that I have now the pleasure to communicate to you.

Saussure's hypothesis, and one of Mr. Knight's, inconsistent with facts.

Selecting parts of the stems of young trees, from which annual branches had sprung in the preceding year, I ascertained by injecting coloured infusions into the stems, through the annual shoots, that the tubes, which descended from the latter, were, at their bases, confined to that side of the stem from which they spring, and to the external annual layer of wood. Deep incisions were then made into the stems of other trees immediately beneath the bases of similar annual shoots, by which I am quite confident, that all communication through the alburnous tubes, with the stem, was wholly cut off: yet the sap passed into the annual shoots in the succeeding spring, all of which lived, and some grew with considerable vigour. I, at the same time, selected many lateral branches, about three lines in diameter, in a nursery of apple trees, which I could easily secure to the stems of the adjoin-

Tubes of annual shoots confined to the external annual layer of wood on the same side. The communication cut off in the stem of some, and the tubes cut through in others.

* Phil. Trans. 1801, p. 345.

ing

ing trees to prevent their being broken. I then made an incision, more than two lines deep in each, on one side, and at the distance of six or seven lines another incision, equally deep, on the opposite side; and as I am quite certain, from the texture of these branches, that the alburnous tubes passed straight through them, I am equally certain, that every alburnous tube was at least once intersected. Yet the sap passed into these branches, and their buds unfolded in the succeeding spring, the incisions having been made in the winter. But I have repeated the same experiment after the leaves have been full grown in the summer, and still the branches have contiuued to live.

The sap still passed, the buds unfolded, and the branches lived.

All naturalists have agreed in stating, that trees perspire most in the summer, when their leaves have attained their full growth, and of course that much sap must ascend at this period; yet at this period the tubes of the alburnum appear dry, and to contain air only; which induced Grew to suppose, that the sap rose in the state of vapour; a supposition by no means admissible. Yet it is, I conceive, evident, that the sap cannot rise as a liquid through dry tubes, nor in any state through intersected tubes; and therefore it appears probable, that it does not rise at all through the tubes of the alburnum, and that these tubes are intended to execute a different office.

When trees perspire most, their alburnous tubes dry.

The sap therefore does not rise through them.

If the sap do not rise through the tubes of the alburnum, it must rise through the cellular substance; yet the passage of any fluid through this has been denied by almost every naturalist, probably because coloured infusions have not been observed to penetrate it, and because many naturalists have considered it as mere compressed medulla. Mirbel, however, contends, that the fluid which generates the new bark exudes from it; and although a fluid, capable of producing the same effects, exudes from the bark, when detached from the alburnum, I am much disposed to coincide with him in opinion, having observed a new bark to be generated on the surface of the cellular substance of pollard oaks, in detached spaces*. And if the sap in sufficient quantity to generate a new bark can pass through the cel-

Consequently it must rise through the cellular substance.

* Phil. Trans. 1807, p. 107; or our Journal, vol. XIX, p. 245.

lular

lular substance of an oak, it appears possible at least, that the whole of the sap may ascend through it. Coloured infusions do not, I think, in any degree, pass through the bark of trees, yet it is evident, that the sap passes readily through it; and therefore should it be proved, that such infusions do not penetrate the cellular substance of the alburnum, the evidence which this circumstance would afford would be very defective.

Coloured infusions not passing, no proof that sap does not.

But coloured infusions will pass through the cellular substance.

Among other experiments which I made to ascertain whether the cellular substance of the alburnum would imbibe coloured infusions, I took off branches of two years old with the annual shoots and leaves attached to them, in the summer, from trees of different species; and I effectually closed the alburnous tubes with a composition formed of calcined oyster shells and cheese*, and this was covered with a mixture of bees wax and turpentine, so as effectually to exclude all moisture. A part of the bark was taken off each branch, in a circle round it, a few lines distant from its lower end, where the tubes had been closed; and each branch was then placed in a decoction of logwood, in a vessel deep enough to cover the decorticated spaces. At the end of twenty hours, or somewhat longer periods, these branches were examined, and the coloured infusion was found to have insinuated itself between the alburnous tubes, in many instances apparently through the cellular substance. This was most obvious in the walnut tree, the young wood of which is very white. The principal object I had in view in making this experiment was, to detect the passages through which I conceived the sap to pass from the bark into the alburnum†.

The sap secretes through the cellular substance of the alburnum.

From the preceding circumstances, I am disposed to infer, that the sap secretes through the cellular substance of the alburnum; and through this I conceive that it must ascend, when the tubes were intersected in the preceding experiments, and in those seasons of the year when the alburnous tubes are empty, though the sap must be rising with great rapidity: and I shall endeavour to show, that the pre-

Effectual cement to stop the bleeding of trees.

* I have found this composition, and this only, to be capable of instantaneously stopping the effusion of sap from the vine, or other tree in the bleeding season.

† Phil. Trans. 1807, p. 107; or Journal, XIX, p. 245.

sence

sence of the sap in the alburnous tubes, during that part of the year in which trees, when wounded, bleed abundantly, does not afford any decisive evidence of the ascent of the sap through those tubes.

Experiments with the sycamore.

In the last spring, when the buds of the sycamore first began to prepare for unfolding, I found, that the sap abounded in the points at the annual branches; and at the same time it flowed abundantly from incisions made into the alburnum near the root. But when similar incisions were made at the distance of eight or ten feet from the ground, not the least moisture flowed; and the tubes of the alburnum appeared to contain air only. I also observed, that the sap flowed as abundantly from the upper as from the under side of the lower incisions, if not more abundantly, and so it continued to flow to the end of the bleeding season.

The sap must therefore have been, by some means, thrown into the tubes above the incisions, for the quantity discharged from them exceeded more than a hundred times that which the tubes could have contained at the time the incisions were made, even had every tube been filled to the extremity of the most distant branch. And, as it has been shown, that the sap can pass up when all the alburnous tubes are intersected, there appears, I think, sufficient evidence, that it must in this case have been raised by some other agent than those tubes.

The sap ascends through the cellular substance.

Through the cellular substance I therefore venture to conclude that the sap ascends; and it is not, I think, difficult to conceive, that this substance may give the impulse, with which the sap is known to ascend in the spring. I have shown, that the bark more readily transmits the descending sap towards the roots than towards the points of the branches*; and if the cellular substance of the alburnum expand and contract, and be so organized as to permit the sap to escape more readily upwards from one cell to another, than in any other direction, it will be readily expelled to the extremities of the branches: and I have shown, that the statement, so often repeated in the writings of naturalists, of a power in the alburnum to transmit the sap with equal facility in op-

The sap not transmitted with equal facility in opposite directions.

* Phil. Trans. 1804, p. 5; or Journal, vol. X, p. 292.

posite

posite directions, and as well through inverted cuttings as others, is totally erroneous†.

The alburnous tubes reservoirs of sap.

If the sap be raised in the manner I have suggested, much of it will probably accumulate in the alburnum in the spring; because the powers of vegetable life are, at that period, more active than at any other season; and the leaves are not then prepared to throw off any part of it by transpiration. And the cellular substance, being then filled, may discharge a part of its contents into the alburnous tubes, which again become reservoirs, and are filled to a greater or less height, in proportion to the vigour of the tree, and the state of the soil and season: and if the tubes which are thus filled be divided, the sap will flow out of them, and the tree will be said to bleed. But as soon as the leaves are unfolded, and begin to execute their office, the sap will be drawn from its reservoirs, and the tree will cease to bleed, if wounded.

These tubes are analogous to the cavities in bones, increasing strength without weight.

The alburnous tubes appear to answer another purpose in trees, and to be analogous, in some degree, in their effects, to the cavities in the bones of animals; by which any degree of strength, that is necessary, is given with less expenditure of materials, or the encumbrance of unnecessary weight; and the wood of many different species of trees is thus made, at the same time, very light, and very strong, the rigid vegetable fibres being placed at greater distances from each other by the intervention of alburnous tubes, and consequently acting with greater mechanical advantage, than they would if placed immediately in contact with each other.

The air in the alburnous tubes conduces to the production of sugar,

I have shown in a former communication, that the specific gravity of the sap increases during its ascent in the spring, and that saccharine matter is generated, which did not previously exist in the alburnum, or in the sap, as it rose from the root: and I conceive it not to be improbable, that the air contained in the alburnous tubes may be instrumental in the generation of this saccharine matter. For I discovered in the last autumn, that much air is absorbed, or at least disappears, during the process of grinding apples for the purpose of making cider, and that during this absorption of

as when apples are ground to make cider,

* Phil. Trans. 1804, p. 5; or Journal, vol. X, p. 292.

air

air, the juice of acid apples becomes very sweet, and acquires many degrees of increased specific gravity; and a similar absorption of air, with corresponding effects, is well known to take place in the process of malting. and in malting.

I shall conclude with observing, that in retracting the opinion I formerly entertained respecting the ascent of the sap in the alburnous tubes, I do not mean to retract any opinion that I have given in former communications respecting the subsequent motion of the sap through the central vessels, the leaves, and bark; or the subsequent junction of the descending with the ascending current in the alburnum: every experiment that I have made has, on the contrary, tended to confirm my former conclusions. Subsequent motion of the sap through the central vessels, leaves, & bark.

I am,

My dear Sir,

Your much obliged obedient servant,

THOMAS ANDREW KNIGHT.

Elton, June 15, 1808.

VI.

Letter on Polygonal Numbers, in Reply to Mr. Gough: *by* P. Barlow, *Esq.*

To the Editor of the Philosophical Journal.

SIR,

MR. Gough is perfectly correct in stating, that I was in possession of his reply previous to the date of my letter inserted in your Journal. The reason I did not think proper to publish it was, that it was written in answer to a private letter, which was not exactly the same in form as that published in your number for October, and consequently an answer to the former could not with propriety have been given as a reply to the latter. Reply to Mr. Gough.

I must now beg leave to be permitted to make a few remarks on the answers of Mr. Gough inserted in your last number;

Reply to Mr. Gough.

number; this gentleman has divided my paper into three objections, which he has pronounced to be *futile*, and I must add without proving them to be so. Mr. Gough will recollect, that in my letter, so far as related to the first four propositions and their corollaries, I did not object to the conclusion, but to the manner of obtaining it; my object was only to show, that an unnecessary number of propositions and corollaries were introduced into the essay, to demonstrate that which needed no demonstration. Mr. Gough does not deny this, except that I have called that a postulate, to which I have given the importance of a theorem, and demonstrated it as such; it is true, as it stands in my letter, it has the appearance of a proposition, but it was unnecessary to give it this form: it might have stood thus;

Definition. Unity is a polygon of every denomination.

Postulate. Let it be granted, that every integer is an aggregate of units, or of polygonals.

Here it is evident that no demonstration is necessary, and had Mr. Gough begun his essay with this postulate, he would at all events have saved himself and readers considerable trouble.

My third, and *only* objection that effects the *truth* of the demonstration, Mr. Gough has evaded, by charging me with putting a false construction upon his words; and now I am under the necessity of retorting the charge. I never said, nor intended to say, that it was necessary to show, that $e = y + t$ can be resolved into $m - f$ polygon in all cases; the question which I proposed was this; "If $e = y + t$ *cannot* be resolved into $m - f$ polygons, how does it appear *from the demonstration*, that $e + f$ can be resolved into m polygons?" This question I again repeat, and unless it can be satisfactorily answered, the theorem will be still without a demonstration, however unwilling Mr. Gough may be to acknowledge it. This gentleman must also be aware, that he cannot be allowed to introduce his examples, which are the moment before derived from the demonstration, to prove the truth of the supposition on which that demonstration is founded.

Mr. Gough appears to have deceived himself by considering only small numbers, in which the polygons, being also

also small, are commonly some of them equal to y, m or unity; let him take a larger number, for example 1520, and resolve into squares; now $e = 1520$, $y = 1444$, and $t = 19\,m$; whereas his demonstration does not include numbers higher than those of the form $e = y + 2\,m$. Reply to Mr. Gough.

It is not my intention to pursue this argument any farther, because however curious the theorem may be as relating to the indeterminate analysis, it is perhaps uninteresting to the greater part of your correspondents. If therefore, Mr. Gough undertake to answer this question, I shall leave it for your mathematical readers to decide for themselves, how far it may be considered as satisfactory.

Yours, &c.

Royal Military Academy, P. BARLOW.
Dec. 9, 1808.

VII.

A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced upon them, by the internal Use of solvent Medicines, from Mr. WILLIAM BRANDE *to* EVERARD HOME, *Esq. F. R. S.*

DEAR SIR,

HAVING availed myself of the opportunity you procured for me of making a chemical examination of the calculi contained in the Hunterian Museum, as well as those in your own collection, I herewith send you an account of what I have done. Calculi in the Hunterian Museum and Mr. Homes,

Should the observations appear to you to throw any new light upon the formation of calculi, I request, that you will do me the honour of laying them before the Royal Society.

The collection, which I have examined, is not only uncommonly large, but the greater part of the specimens have histories of the case annexed to them. very numerous, and in general with

 This

their histories annexed.

This circumstance enabled me not only to ascertain the situations in which the calculi were found, but likewise many of the circumstances attendant on their formation.

I have therefore endeavoured to form an arrangement upon these principles, with a view to render the subject more clear and perspicuous.

SECT. I. *Of Calculi formed in the Kidnies, and voided without having afterwards undergone any Change in the urinary Passages.*

Calculi of the kidneys.

These have the following properties:

They are of a brownish yellow colour, sometimes of a grayish hue, which seems to arise from a small portion of dry mucus adhering to their surface.

They are entirely soluble in a solution of pure potash, and during their solution they seldom emit an odour of ammonia.

When heated to dryness, with nitric acid, the residuum is of a fine and permanent colour.

Contain animal matter.

When exposed to the action of the blow-pipe, they blacken and emit a strong odour of burning animal matter, very different from that of pure uric acid. This arises from a variable proportion of animal matter which they contain, and which occasions the loss in the analysis of these calculi. Its relative quantity is liable to much variation, as may be seen from the following statements,

A calculus from the kidney, weighing seven grains, was dissolved in a solution of pure potash. A quantity of muriatic acid (rather more than sufficient for the saturation of the potash) was added, and the precipitate of uric acid thus obtained weighed when dry 4·5 grains. No other substance, except animal matter, which was evident on attempting to obtain the muriate of potash, could be detected, consequently the composition of this calculus was as follows:

	Grs.
Uric acid	4·5
Animal matter	2·5
	7·0

This

This is the largest proportion of animal matter which I have met with.

A small calculus from the kidney, weighing 3·7 grains, afforded by a like treatment 3·5 grains of uric acid, so that it was nearly a pure specimen of that substance. Sometimes in very small quantity.

The largest calculus of this kind which I have examined weighed seventeen grains; much larger ones have been found, but there is no evidence of their not having remained in the urinary passages for some considerable time. Thus Dr. Heberden mentions one weighing twenty-eight grains*.

It often happens that the ingredients are not united together so as to form a calculus, but are voided in the state of a fine powder, commonly termed sand. This consists either of uric acid, or of the ammoniaco-magnesian phosphate, alone, or with the phosphate of lime. Sand. Phosphates found in this,

I am induced to believe, that the last mentioned substances, although the production of the kidnies, and held in solution, are never met with in a separate state, till the urine has been at rest, and therefore calculi from the kidnies are never composed of the phosphates. but not in the calculi.

In a few instances, calculi from the kidnies composed of oxalate of lime are voided; but this is a very rare occurrence. Of three preserved in the Hunterian collection, two are extremely small and hard, having an appearance of being made up of several smaller calculi, of a dark brown colour. The third is of the size of a small pea, its surface smooth, and of a gray colour, not very hard. Oxalate of lime.

SECT. II. *Of Calculi which have been retained in the Kidney.*

When one or more of the calculi described in the preceding section are detained in the infundibula or pelvis of the kidney, it frequently happens, that they increase in that situation to a considerable size. Calculi detained in the kidney.

This increase is of two kinds.

1. Where there is a great disposition to the formation of uric acid, the calculus consists wholly of that substance and animal matter, so as frequently to form a complete cast of the pelvis of the kidney.

* Comment on the Hist. and Cure of Diseases, 3d. edit. p. 88.

2. Where

Phosphates in these.

2. Where there is less disposition to form uric acid, the external laminæ are composed of the ammoniaco-magnesian phosphate, and phosphate of lime.

In one instance, a small uric calculus has been deposited in the kidney in such a situation, that its upper surface was exposed to a continual stream of urine, upon which beautiful crystals of the triple phosphate had been deposited. It would therefore seem, that, under common circumstances, a stream of urine passing over a calculus of uric acid has a tendency to deposit the phosphate upon it.

Sect. III. *Of Calculi of the urinary Bladder.*

Calculi of the bladder.

Calculi met with in the bladder are of four kinds.

1. Those formed upon nuclei of uric acid from the kidney.

2. Those formed upon nuclei of oxalate of lime from the kidney.

3. Those formed upon sand, or animal mucus, deposited in the bladder.

4. Those formed upon extraneous bodies introduced into the bladder.

They were arranged under the following divisions.

Calculi of uric acid chiefly.

1. Calculi, which, from their external appearance, consist chiefly of uric acid.

These calculi vary in colour from a deep reddish brown, to a pale yellowish brown.

They are either entirely soluble in a solution of pure potash, or nearly so.

During their solution they frequently emit the odour of ammonia.

When acetic acid is added to their alkaline solution, a precipitate possessing the properties of uric acid is obtained.

Calculi of phosphates chiefly.

2. Calculi composed chiefly of the ammoniaco-magnesian phosphate, or of phosphate of lime, or of mixtures of the two.

These calculi are externally of a whiter appearance than the former.

Some perfectly white, others gray, occasionally exhibiting small prismatic crystals upon their surface; others again soft and friable, a good deal resembling chalk. They

are

are farther characterised by their solubility in dilute muriatic acid.

3. Calculi containing oxalate of lime; commonly called mulberry calculi.

With oxalate of lime, or mulberry calculi.

These are distinguished by the difficulty with which they dissolve in dilute acids, by their hardness, and by leaving pure lime, when exposed to the action of the blow-pipe.

In the examination of these calculi, I was struck with the small number of those strictly belonging to the first division, having been led, from the account of Fourcroy and Vauquelin*, and the experiments of Dr. Pearson†, to believe that calculi, composed of pure uric acid, were by no means unfrequent.

The greater number of the calculi examined by the former chemists are stated to be completely soluble in the fixed alkaline lies; and of three hundred examined by Dr. Pearson, a large proportion is said to consist of uric acid.

Composition of different calculi.

The following is a statement of the composition of the different calculi found in the bladder which I have examined.

16	were composed of uric acid.
45	——— uric acid with a small relative proportion of the phosphates.
66	——— the phosphates, with a relatively small proportion of uric acid.
12	——— of the phosphates entirely.
5	——— of uric acid, with the phosphates and nuclei of oxalate of lime.
6	——— chiefly of oxalate of lime.
150	

Sawn in two and a portion filed off.

To injure these calculi as little as possible, they were carefully cut through with a fine saw, and a portion of the whole cut surface removed by a file; in this way all the different ingredients of the calculi were obtained.

More loss from those of the

In the experiments upon uric calculi from the bladder, I found, in most instances, a far more considerable loss in at-

* Annales de Chimie, xxxii, 218.

† Philos. Trans. 1798. p. 37.

tempting

bladder than those of the kidney.

tempting to obtain their pure uric acid, than in the kidney calculi, which led me to suppose, that they contained urea; and that the presence of this substance, with some of the salts of urine, and with small portions of the ammoniaco-magnesian phosphate, was the cause of the occasional evolution of ammonia when treated with the fixed alkalis, and of their easy solubility in those substances.

One supposed to consist of urate of ammonia digested in water.

To determine this point, a small calculus, weighing twenty-five grains, and of the species commonly supposed to consist of urate of ammonia*, was digested for two hours with water in a very moderate heat. The water which had assumed a pale yellow colour was filtered off, and fresh water added to the residuum three successive times, when it appeared, that every thing soluble in that fluid was separated. The insoluble part of the calculus, being now carefully dried and weighed, was found to have lost 5·5 grains.

Solution evaporated.

The aqueous solution was evaporated by a gentle heat, nearly to dryness, and a substance was obtained having all the properties of urea, in combination with a small portion of muriate of ammonia, and of the ammoniaco-magnesian phosphate.

Another digested in alcohol.

Sixty grains of another calculus of a considerable size, supposed from a superficial analysis, to consist of nearly pure urate of ammonia, were digested at a low temperature in one ounce of alcohol. In an hour the alcohol was decanted off, and fresh portions were added successively, as long as it appeared to act upon the calculus, which, after having been carefully dried in a temperature below 212°, weighed 54·8 grains, so that 5·2 grains had been taken up by the alcohol.

Solution evaporated.

On evaporating the alcoholic solutions, a substance was obtained having all the properties of urea, with a small portion of saline matter, probably muriate of ammonia, as by the addition of potash a slight ammoniacal odour was perceptible; its quantity however was too minute for accurate examination.

* Fourcroy observes, that urate of ammonia is easily detected by its rapid solubility in the fixed alkalis, and the odour of ammonia, which is perceived during its solution.—Vide Thomson's Syst. of Chem. vol. v, p. 691.

The remaining portion of the calculus, weighing 54·8 grains, was treated with small portions of acetic acid, by which 6 grains of the ammoniaco-magnesian phosphate were obtained.

Treated with acetic acid.

The part of the calculus remaining after this treatment, weighing 48·8 grains, was perfectly soluble in a solution of pure potash; it emitted no ammoniacal odour when acted upon by the alkali, and possessed the properties of pure uric acid.

Residuum dissolved in potash.

The following therefore is the composition of this calculus.

Component parts.

	Grains.
Urea, and muriate of ammonia	5·2
Ammoniaco-magnesian phosphate ..	6·
Uric acid	48·8
	60·

From these and many similar experiments upon other calculi, hitherto generally supposed to consist of urate of ammonia, I am induced to believe, that the evolution of ammonia depends in all instances upon the decomposition of the ammoniacal salts contained in the calculus, more especially of the ammoniaco-magnesian phosphate, and that no substance, which can be called *urate of ammonia*, exists in calculi.

No urate of ammonia in alcohol.

The mulberry calculus (oxalate of lime) I have but rarely met with. In those preserved in the Hunterian Collection there is a large relative proportion of phosphate of lime, and of uric acid. The purest of them afforded

Mulberry calculi.

	Grs.
Oxalate of lime	65·
Uric acid	16·
Phosphate of lime	15·
Loss in animal matter	4·
	100·

When calculi of the urinary bladder increase to a very large size, they are generally composed of two or even three of the above mentioned varieties, the ammoniaco-magnesian phosphate

Very large calculi seldom homogeneous.

phosphate being situate externally, and in the greatest abundance.

One of 23 oz. 26 grs.

The largest calculus, which I have seen, weighed, when recently removed from the bladder, twenty-three ounces and twenty-six grains. It consisted of a large mulberry or oxalate of lime calculus, the nucleus of which was uric acid, surrounded by a considerable quantity of the ammoniaco-magnesian phosphate in a very pure state.

One of 15½ oz.

Another very large calculus, weighing fifteen ounces and a half, consisted of a nucleus of uric acid, enveloped in the ammoniaco-magnesian phosphate, not however pure, but intersected by several laminæ of uric acid.

Four distinct substances rare.

Four distinct substances are extremely rare in calculi; I have seen one in which the uric acid, the ammoniaco-magnesian phosphate, the phosphate of lime, and the oxalate of lime, were all in perfectly separate and distinct layers.

Four formed on foreign nuclei.

Four calculi, having the following extraneous substances for their nuclei were examined.

1. A common garden pea.
2. A needle.
3. A hazel nut.
4. A part of a common bougie.

In the two first instances, the calculous depositions were of a pale gray colour, inclining to white; soft and friable in their texture, and entirely soluble in muriatic acid.

The composition of the first was as follows;

	Grs.
Phosphate of lime	65·
Ammoniaco-magnesian phosphate	28·
Loss	7·
	100·

Of the second;

Phosphate of lime	45·
Ammoniaco-magnesian phosphate	38·
Oxalate of lime	12·
Loss	5·
	100·*

* It appears, that in this case there had been an accidental disposition to the formation of oxalate of lime.

The

The deposition of calculous matter upon the bougie was covered with blood, and in very small quantity, the bougie having been removed by an operation soon after it had passed into the bladder. It appeared to consist chiefly of phosphate of lime.

The incrustation upon the hazel nut was also destitute of uric acid.

Sect. IV. *Of the Calculi of the Urethra.*

All those that were examined had escaped from the bladder while very small, and had afterward lodged in the membranous part of the urethra, where they had increased in size, and formed a cavity, in which they were more or less imbedded. Calculi of the urethra.

Two of these calculi were broken.

The fragments consisted in one instance of ammoniaco-magnesian phosphate, and phosphate of lime, with a small portion of uric acid: and in the other the fragments were composed entirely of the ammoniaco-magnesian phosphate.

The third calculus was of a very remarkable appearance; its form being that of a perfect sphere, about half an inch in diameter. It was coated with small but very regular crystals of the triple phosphate in its purest state. On account of the singularity of the form and external appearance of this calculus, it was not sawn through; the nucleus, in all probability, is a small kidney calculus, which lodging in the urethra has become coated with triple phosphate.

Sect. V. *Analysis of Calculi from other Animals.*

1. The horse. Calculi of the horse.

A. *From the kidney.*

A very large calculus, from the kidney of a horse, was composed of From the kidney.

Phosphate of lime	76
Carbonate of lime	22
	98

B. *From the bladder.*

This calculus was also of a large size; its weight, when perfectly dry, nine ounces and a half; its external surface From the bladder.

very

very irregular, of a reddish brown colour, and covered with minute crystals of the ammoniaco-magnesian phosphate. On making a section of it, the internal structure exhibited a radiated appearance, and was of a light brown colour. It consisted of

Phosphate of lime	45·
Ammoniaco-magnesian phosphate	28·
Animal matter	15·
Carbonate of lime	10·
	98·

In another case the bladder of a horse was found to be nearly full of sand, the composition of which was as follows;

Phosphate of lime	60·
Carbonate of lime	40·
	100·

Calculi of the ox.

2. The ox.

A number of small calculi, from the size of a pea downwards, are not unfrequently found in the bladder of the ox. Those in the Hunterian Collection are of a pale brown colour, and of the size just mentioned; some of them have the mulberry appearance.

They consist of carbonate of lime and amimal matter, which last substance retains the form of the calculus, after it has been acted upon by diluted acids.

Of the sheep.

3. The sheep.

A calculus from the kidney of a sheep was composed of

Phosphate of lime	72·
Carbonate of lime	20·
Animal matter	8·
	100·

Of the rhinoceros.

4. The rhinoceros.

The urine of this animal is exceedingly turbid at the time it is voided, and when allowed to remain at rest, deposites a very large proportion of sediment, which consists of carbonate of lime, with small portions of phosphate of lime and animal matter.

5. The

5. The dog. Of the dog.

A large calculus from the bladder of a dog twenty years old, weighing sixteen ounces, was extremely hard, and of a gray colour; when cut through, it exhibited a nucleus about the size of a hazel nut, partly made up of concentric layers of phosphate of lime, and partly of crystals of the ammoniaco-magnesian phosphate. The part of the stone surrounding the nucleus consisted of

Phosphate of lime	64·
Ammoniaco-magnesian phosphate	30·
Animal matter	6·
	100·

Sand taken from a dog's bladder was of a gray colour, and contained

Carbonate of lime	20·
Phosphate of lime	80·
	100·

6. The hog. Of the hog.

A calculus from the bladder of this animal weighed nineteen drachms; it was of a pale gray colour inclining to white, and so hard that it was with difficulty cut through. Its internal structure was uniform, and there was no appearance of a nucleus. It was composed of

Carbonate of lime	90·
Animal matter	10·
	100·

7. The rabbit. Of the rabbit.

A calculus from the rabbit's bladder weighing four drachms, was of a dark gray colour, and appeared as if composed of several smaller calculi. It consisted of

Phosphate of lime	39·
Carbonate of lime	42·
Animal matter	19·
	100·

SECT.

Sect. VI. *General Inferences.*

General conclusions.

It appears from the preceding observations, that calculi formed in the kidnies, and immediately voided, are almost always composed of uric acid; and that the phosphates are very frequent ingredients in calculi of the bladder, more especially in those, which, from their situation, have been exposed to a continual current of urine: they also uniformly are deposited upon extraneous substances introduced into the bladder, but appear never to form small kidney calculi.

Fit of the gravel.

In what is commonly called a fit of the gravel, a small uric calculus is formed in the kidney, and passes along the ureter into the bladder.

Formation of the stone in the bladder.

It is found from observation, that for some time after a stone has passed from the kidney, the urine is generally unusually loaded with uric acid, and deposites that substance upon the nucleus now in the bladder. When this period, which is longer or shorter in different individuals, has elapsed, the subsequent addition to the calculus consists principally of the phosphates.

Where the disposition therefore to form uric acid in the kidnies is very great and permanent, the calculus found in the bladder is principally composed of uric acid; but where this disposition is weak and of short duration, the nucleus only is uric acid, and the bulk of the stone is composed of the phosphates.

Where the increased secretion of uric acid returns at intervals, the calculus is composed of alternate layers of uric acid and the phosphates.

Other small calculi being formed in the kidney, they make their way into the bladder, and afford fresh nuclei; so that several calculi are sometimes found in the same bladder, and their composition is usually nearly the same.

In other cases it happens, that a constant increased secretion of uric acid is going on from the kidnies, only in small quantity, which will be more uniformly mixed with the phosphates deposited in the bladder, and where the uric acid predominates, the species of calculus, denominated improperly *urate of ammonia*, will be produced.

Formation of

We are entirely ignorant of the cause of the formation of

the oxalate of lime, or mulberry calculus. I have frequently looked for oxalate of lime in the urine of calculous patients, but have never been able to detect it; and as it does not exist in healthy urine, it must be regarded as a morbid secretion. Its mode of formation seems to resemble that of uric acid, since small kidney calculi, composed of oxalate of lime, have in a few instances been voided; and in these cases, as far as my own inquiries go, the persons have been much less liable to a return of the complaint, than where uric calculi have been voided. the mulberry calculus.

In some rare instances we meet with calculi of the bladder which are destitute of uric acid, and of oxalate of lime, the nucleus being composed of a little loosely agglutinated ammoniaco-magnesian phosphate, and the whole calculus consisting of that substance, with variable portions of phosphate of lime: in two cases I have met with calculi of this kind, composed of the triple phosphate only: they seem to be entirely formed in the bladder.

Action of solvents.

Having taken this short view of the formation of calculi, I shall now inquire into the action of solvents, employed either with a view of effecting their solution, or of preventing their formation and increase.

Solvents are of two kinds.

1. Alkaline. 2. Acid.

In the exhibition of these, the practitioner is usually guided by the chemical composition of the calculous matter voided by urine.

Gravel of two kinds.

The different kinds of gravel, voided by persons labouring under calculous complaints, may be classed in two divisions.

1. *Uric acid*, either in a pure state, or with a very small proportion of the phosphates.

2. *The phosphates*, either pure, or with a small proportion of uric acid.

Uric acid.

The first species, which generally appears in the form of minute crystalline grains, of a reddish brown colour, or of an impalpable brown powder, is either entirely soluble in pure alkaline solutions, not emitting an ammoniacal odour, in which case it consists of pure uric acid: or it does emit an ammoniacal odour, and is not entirely soluble, in which case

case it contains the triple phosphate of ammonia and magnesia.

Effect of alkalis.

When this substance is observed in the urine, the alkalis are recommended. They are exhibited either in a pure state, or as carbonates, and in each instance the uric sediment generally diminishes rapidly, and during the continued use of alkaline medicines, occasionally disappears altogether.

It however frequently happens, that the matter voided is not diminished in quantity by the use of alkalis, but that its form and composition are altered, and that it assumes the appearance of a gray powder, and is composed of uric acid with variable portions of the ammoniaco-magnesian phosphate.

Prevent the increase of a calculus.

From these facts therefore it cannot be doubted, that the internal exhibition of alkalis often prevents the formation of uric acid, and hence must likewise prevent the increase of a calculus in the bladder, as far at least as uric acid is concerned; but it has also been supposed, that the alkalis are capable of acting upon the stone itself, and even of effecting its complete solution. It is true, that, if we immerse a calculus composed of uric acid in a dilute solution of caustic alkali, it will be slowly acted upon, and after some time entirely dissolved. If however we attend to what would take place in the body, we shall find the circumstances very different.

Caustic alkali dissolves a calculus out of the bladder.

Alkaline carbonates do not.

That alkaline carbonates and subcarbonates exert no action upon uric acid, I consider to be completely established, both by the experiments of several eminent chemists, and those I have myself made upon the subject; and as there is at all times a quantity of uncombined acid in the urine, it follows, that, although the alkali may arrive at the kidnies in its pure state, it will there unite with the uncombined acid, and be rendered incapable of exerting any action upon the calculus in the bladder. Beside phosphoric acid, the urine always contains a quantity of uncombined carbonic acid; this is proved by placing a quantity of recently voided urine under the receiver of an air pump; during the exhaustion, a large quantity of carbonic acid gas makes its escape: and when urine is distilled at very low temperatures, carbonic

Alkalis therefore cannot act on the stone in the bladder.

carbonic acid gas is given off: and also, when lime water is poured into urine, a precipitate appears, consisting of phosphate and carbonate of lime.

Limewater more objectionable.

Lime water, on account of the insoluble compound which lime forms with carbonic and phosphoric acids, is even more objectionable as a solvent, than the alkalis.

How far palliative.

It may however be said, that, if these means prevent the increase of a calculus, material relief is afforded to the patient. How far the exhibition of alkaline remedies can be recommended upon these grounds will appear, when the circumstances, which attend the formation of the second species of calculous sediment or deposition in the urine, are considered.

Ammoniaco-magnesian phosphate.

The ammoniaco-magnesian phosphate appears under two forms: it is either voided in a solid state, or in solution. In the former case it bears a good deal of resemblance to a white sand, and is frequently mixed with variable proportions of phosphate of lime. In the latter it makes its appearance after the urine has remained undisturbed for some hours in an open vessel, generally in the form of a fine pellicle, or of crystalline laminæ, which when collected and dried bear some resemblance to boracic acid.

Its putting on this form is accounted for, from its being held in solution in the first instance by carbonic acid, and as this flies off, the triple salt makes its appearance. If a portion of the urine be preserved in a phial closely stopped, the carbonic acid cannot escape, and consequently no phosphate is observed to separate. There is also a quantity of phosphoric acid present, which keeps another portion of the ammoniaco-magnesian phosphate, and also some lime (in the state of superphosphate of lime) in solution.

Injurious effects of alkalis.

It is therefore obvious, that, whenever the urine is deprived of a portion of the acid which is natural to it, the deposition of the triple phosphate, and phosphate of lime, more readily takes place: this is effected by the exhibition of the alkalis.

It may therefore be asserted, that, although alkaline medicines often tend to diminish the quantity of the uric acid, and thus to prevent the addition of that substance in its pure state

state to a calculus in the bladder; they favour the deposition of the phosphates.

They reach the bladder.

It cannot be doubted, that the alkalis reach the bladder, since in cases where large doses of subcarbonate of potash have been exhibited, I have seen evident traces of it in the urine.

Use of acids.

Where the phosphates only are voided, it has been proposed to dissolve the calculus by the exhibition of acids, and more especially the muriatic acid.

During the use of the muriatic acid, the phosphates are either diminished or disappear altogether; and even sometimes the urine acquires an additional acidity; a solution of that part of the calculus, which consists of the phosphates, might therefore be expected; but even then the nucleus of uric acid would remain, and thus a great deal of time would be lost without any permanent advantage.

I have also occasionally remarked, that during the use of acids, the uric acid reappears, and even seems to be augmented in quantity.

Injection of solvents.

Attempts have been made at different times to effect the solution of calculi, by the injection of solvents into the bladder. This subject has been more lately revived by Fourcroy and Vauquelin, who, in their paper on the composition of calculi, lay down rules for its practice. Independent, however, of the impossibility of ascertaining the composition of the calculus with sufficient accuracy, it is obvious, that were the composition of the surface of the calculus known, the frequent introduction of an instrument into the bladder, and the long continuance of the process which would be necessary, even where the calculi are small, are insurmountable objections; and whenever this mode of treatment has been adopted, it has speedily been relinquished, as it always aggravates the sufferings of the patient.

Real use of alkalis.

It has been shown, that, in the majority of cases, the nuclei of calculi originate in the kidnies, and that of these nuclei by far the greater number consist of uric acid; the good effects therefore, so frequently observed during the use of an alkali, arise, not from any actual solution of calculous matter, but from the power which it possesses of diminishing the secretion of uric acid, and thus preventing the enlargement of

of the calculus, so that, while of a very small form, it may be voided by the urethra.

VIII.

Some Observations on Mr. Brande's *Paper on Calculi. By* Everard Home, *Esq. F. R. S**.

THAT calculi in the human bladder are not dissolved by the internal use of alkaline medicines, is an opinion which I have long entertained, but the grounds of failure, so clearly pointed out by Mr. W. Brande, were not known to me: I only knew from experience, that, to whatever extent the medicines are given, no such effect takes place. The circumstance of the exterior laminæ of calculi extracted from patients, who had persevered in a course of alkaline preparations, having been found softer than the parts towards the centre, has always been considered as a proof of the action of the medicines upon the calculus, and led to the belief, that where the stone was small, it might be wholly dissolved. This, however, Mr. W. Brande has now proved to be a deception, and that the soft part is not a portion of the original calculus, but a newly formed substance, in which the uric acid is not deposited in crystals, but mechanically mixed with the phosphates and the animal mucus in the urine.

Inefficacy of alkaline medicines.

Ground of the contrary opinion

a deception.

Having met with cases, which confirm Mr. W. Brande's observations, it will be satisfactory to state them, as they may assist in doing away many erroneous notions generally entertained on this subject.

Cases confirm this.

The opinion, that calculi in the human bladder have been entirely dissolved, has received its principal support from instances having occurred, and those by no means few in number, where the symptoms went entirely away while the patients were using alkaline medicines, and never afterward returned. This evidence appears to be very strong, but it will be found from the following cases, that it is not so in reality; since the fallacy has been detected in all the instances in which an opportunity was afforded of examining

Apparently strong evidence

a mere fallacy.

* Phil. Trans. for 1808, p. 244.

the

the bladder after death. Two of these I shall particularly notice, because they were published during the patient's life time in proof of the stone having been dissolved.

Case illustrating this.

Both patients were great sufferers from the symptoms of stone for many years; but when they arrived at the age of sixty-eight, or thereabout, the symptoms entirely left them. The one had been taking the saline draught in a state of effervescence, under the direction of the late Dr. Hulme: the cure was attributed to this medicine, and the case was published in proof of its efficacy. When the patient died, I examined the bladder, and found twenty calculi; the largest of the size of a hazel nut, the others smaller. It appeared, that the going off of the symptoms had arisen from the posterior lobe of the prostate gland having become enlarged (a change which it frequently undergoes about that period of life,) and having formed a barrier between the calculi and the orifice of the bladder, so that they no longer irritated that part either in the act of making water, or in the different movements of the body, but lay in the lower posterior part of the bladder without producing any disturbance. Their number prevented the pressure from being great upon any one part of the intestine immediately behind the bladder, and their motion on one another rendered their external surface smooth, and probably prevented their rapid increase.

Another.

The other patient was under a course of Perry's lixivium; and when the symptoms went away, he published the case in proof of the efficacy of that medicine in dissolving the stone. I examined the bladder after death, and found fourteen calculi; the largest of the size of a nutmeg, the others smaller. There was the same enlargement of the posterior lobe of the prostate gland, and the calculi were exactly under the same circumstances as in the former case.

In several cases stones not felt.

In several cases, in which I have examined the body after death, calculi have been found enclosed in cysts, formed between the fasciculi of the muscular coat of the bladder, so as to be entirely excluded from the general cavity, and therefore had not produced any of the common symptoms of stone. I have seen in the same bladder, two, three, and even four such cysts, each containing a calculus of the size of a walnut.

It

It is a circumstance deserving notice, that in the case, which gave celebrity to Mrs. Stevens's medicine, and procured her a remuneration from Parliament, the bladder was not examined after death. Mrs. Stevens.

That calculi in the bladder do sometimes increase, while the patient is using alkaline medicines, is fully proved by the following examples, which also show, that the uric acid and phosphates are formed in different proportions, according to the peculiarities of the constitution. Calculi sometimes increase during the use of alkalis.

A gentleman who suffered from symptoms of stone was sounded, and a stone was found in his bladder. I put him on a course of alkaline medicines, and he voided a small compact calculus, composed of uric acid, and evidently formed in the kidney. He was desired to persist in the use of the medicines, which he did at intervals for four or five years, suffering occasionally in a slight degree, but he did not pass any more calculi. He died at the age of seventy-five. On examining the bladder, its whole cavity, (the capacity of which was equal to a pint measure) was completely filled with soft, light, spongy calculi, three hundred and fifty in number, and of different sizes, from that of a walnut to a small pea. They were composed of a mixture of uric acid in powder, the phosphates, and animal mucus; and differed so much from the calculus voided soon after the patient begun the use of alkalis, that they appear to have been formed after that period in the manner mentioned by Mr. W. Brande. Instance of this.

A gentleman, who was found to have a stone in his bladder, was persuaded, that it was so small, that it might be dissolved, and with this view he took the fossil alkali, both in its caustic and mild state, for about three months; but at the end of that period the symptoms were increased, and he submitted to have it extracted by an operation. On examining the calculus after it was extracted, the external part, for the thickness of $\frac{1}{10}$ of an inch, was entirely composed of triple phosphate, in a state of perfect spiculated crystals, so as to present a very rough irritating surface to the internal membrane of the bladder, while the inner parts of the calculus were made up of a mixture of uric acid and phosphates, so that the alkali had prevented the formation of Another.

of uric acid, but the phosphates were deposited more rapidly than before.

Alkalis do not always counteract the formation of uric acid.

A gentleman, in whose urine the uric acid appears in a solid form immediately after it is voided, has the same appearance in the urine, even when nine drachms of soda dissolved in water impregnated with carbonic acid are taken in twenty-four hours; so that in this instance the alkali does not even counteract the formation of uric acid.

IX.

Electro-Chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.*

(Concluded from Vol. XXI, p. 383.)

V. *On the production of an Amalgam from Ammonia, and on its Nature and Properties.*

Deoxidation and amalgamation of the compound base of ammonia.

IN the communication from Professor Berzelius and Dr. Pontin, which I have already referred to, a most curious and important experiment on the deoxidation and amalgamation of the compound basis of ammonia is mentioned, which these ingenious gentlemen regard as a strict proof of the idea I had formed of its being an oxide with a binary basis.

Mercury, negatively electrified in the Voltaic circuit, is placed in contact with solution of ammonia. Under this agency it gradually increases in volume, and, when expanded to four or five times its former dimensions, becomes a soft solid.

And that this substance is composed of the deoxigenated compound basis of ammonia and mercury, they think is proved; 1. By the reproduction of quicksilver and ammonia, with the absorption of oxigen, when it is exposed to air; and secondly, by its forming ammonia in water, while hidrogen is evolved, and the quicksilver gradually becomes free.

An

An operation, in which hidrogen and nitrogen exhibit metallic properties, or in which a metallic substance is apparently composed from its elements, cannot fail to fix the attention of chemists: and the peculiar interest, which it offered in its relations to the general theory of electrochemical science, induced me to examine the circumstances connected with it minutely and extensively.

The process repeated.

In repeating the process of the Swedish chemists, I found, that to form an amalgam from fifty or sixty grains of mercury, in contact with saturated solution of ammonia, required a considerable time, and that this amalgam greatly changed even in the short period required for removing it from the solution.

I was however able, in this mode of operating, to witness all the results they have stated, and I soon found simple and more easy means of producing the effect, and circumstances under which it could be more distinctly analysed.

Ammonia probably in its nascent state would be acted on more readily.

The experiments, which I have detailed in the Bakerian lecture for 1806, proved, that ammonia is disengaged from the ammoniacal salts at the negative surface in the Voltaic circuit; and I concluded, that, under this agency, it may be acted on in what is called the nascent state, when it was reasonable to conclude it would be more readily deoxigenated and combined with quicksilver.

50 grs. of mercury placed in a cavity in muriate of ammonia and electrified.

On this view of the subject, I made a cavity in a piece of muriate of ammonia; into this a globule of mercury, weighing about fifty grains, was introduced. The muriate was slightly moistened, so as to be rendered a conductor, and placed on a plate of platina, which was made positive in the circuit of the large battery. The quicksilver was made negative by means of a platina wire. The action of the quicksilver on the salt was immediate; a strong effervescence with much heat took place. The globule in a few minutes had enlarged to five times its former dimensions, and had the appearance of an amalgam of zinc; and metallic crystallizations shot from it, as a centre, round the body of the salt. They had an arborescent appearance, often became coloured at their points of contact with the muriate; and when the connection was broken, rapidly disappeared,

Amalgam produced.

peared, emitting ammoniacal fumes, and reproducing quicksilver.

Carbonate of ammonia produced a similar amalgam,

When a piece of moistened carbonate of ammonia was used, the appearances were the same, and the amalgam was formed with equal rapidity. In this process of deoxidation, when the battery was in powerful action, a black matter formed in the cavity, which there is every reason to believe was carbonaceous matter from the decomposition of the carbonic acid of the carbonate*.

and some carbon.

Potassium, sodium, &c. employed to deoxidate ammonia without electricity.

The strong attraction of potassium, sodium, and the metals of the alkaline earths for oxigen, induced me to examine whether their deoxidating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and the result was very satisfactory.

When mercury, united to a small quantity of potassium, sodium, barium, or calcium, was made to act upon moistened muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

The amalgam not pure.

As in these cases, however, a portion of metal used for the deoxidation always remained in union in the compound; in describing the properties of the amalgam from ammonia, I shall speak only of that procured by electrical means.

Its properties.

The amalgam from ammonia, when formed at the temperature of 70° or 80, is a soft solid, of the consistence of butter: at the freezing temperature it becomes firmer, and a crystallized mass, in which small facets appear, but having no perfectly defined form†. Its specific gravity is below 3, water being one.

When thrown into water it produces a quantity of hydrogen,

* The black matter which separates at the negative surface in the electrical experiments on the decomposition of potash or soda, and which some experimenters have found it difficult to account for, is I find carbonaceous, and dependent upon the presence of carbonic acid in the alkali. [See our Journal, vol. XIX, p. 156, and 307]

† From the facet I suspect the form to be cubical. The amalgam of potassium crystallizes in cubes as beautiful, and in some cases as large, as those of bismuth.

drogen, equal to about half its bulk, and in consequence of this action the water becomes a weak solution of ammonia.

When it is confined in a given portion of air, the air enlarges considerably in volume, and the quicksilver reappears. Ammoniacal gas, equal to one and a half or one and three fifths of the volume of the amalgam is found to be produced, and a quantity of oxigen equal to one seventh, or one eighth of the ammonia disappears†.

When thrown into muriatic acid gas, it instantly becomes coated with muriate of ammonia, and a small quantity of hidrogen is disengaged.

In sulphuric acid it becomes coated with sulphate of ammonia and sulphur.

The metal could not be obtained separate.

I attempted by a variety of modes to preserve this amalgam. I had hoped by submitting it to distillation out of the contact of air, or water, or bodies which could furnish oxigen, to be able to obtain the deoxigenated substance, which had been united to the quicksilver in a pure form; but all the circumstances of the experiment opposed themselves to such a result.

It is well known to persons accustomed to barometrical experiments, that mercury, after being once moistened, retains water with great perseverance, and can only be freed from it by boiling; and in the cases of the decomposition of ammonia, when a soft amalgam had been kept continually moist, both internally and externally for some time, it could not be expected, that all the water adhering to it should be easily removed.

I wiped the amalgam as carefully as possible with bibulous paper; but even in this process a considerable portion of the ammonia was regenerated; I attempted to free it from moisture by passing it through fine linen, but a complete decomposition was effected, and nothing was obtained but pure quicksilver.

The whole quantity of the basis of ammonia combined in sixty grains of quicksilver, as is evident from the statements that have been made, does not exceed $\frac{1}{100}$ part of a grain,

† This experiment confirms the opinions I have stated concerning the quantity of oxigen in ammonia; but as water is present, as will be immediately shown, the data for proportions are not perfectly correct.

and

and to supply oxigen to this scarcely $\frac{1}{1000}$ part of a grain of water would be required, which is a quantity hardly appreciable, and which merely breathing upon the amalgam would be almost sufficient to communicate.

The amalgam quickly decomposed.

Hence, when an amalgam, which had been wiped by means of bibulous paper, was introduced into naphtha, it decomposed almost as rapidly as in the air, producing ammonia and hidrogen.

In oils it evolved hidrogen, and generated ammoniacal soap; and when it was introduced into a glass tube, closed by a cork, gas was rapidly formed, and the mercury remained free; and this gas, when examined, was found to consist of from about two thirds to three fourths ammonia, and the remainder hidrogen*.

That more moisture sometimes existed attached to the amalgam, when wiped as dry as possible by bibulous paper, than was sufficient for the effect of decomposition, I soon found by an experiment of distillation.

Distilled.

About a quarter of a cubic inch of an amalgam nearly solid was wiped very dry, and introduced into a small tube: in this tube it was heated till the gaseous matter had expelled the quicksilver; the tube was then closed, and suffered to cool, when moisture, which proved to be a saturated solution of ammonia, had precipitated upon it.

The triple amalgams

I have mentioned, that the amalgams obtained from ammonia by means of the metals of the fixed alkalis or alkaline earths seemed to contain much more ammoniacal basis in combination than those procured by electricity: and when they are combined with the metals of the fixed alkalis or of the earths in any considerable quantities, they are much more permanent.

may be preserved some time.

Triple compounds of this kind, when carefully wiped, scarcely produce any ammonia under naphtha, or oil, and may be preserved for a considerable time in closed glass tubes, a little hidrogen being the only product evolved from them.

Heated over mercury.

I heated a triple amalgam obtained from ammonia by potassium

* In the experiment of the action of the amalgam upon air, the oxigen is probably absorbed by nascent hidrogen, and reproduces water, which is dissolved by the ammonia.

potassium, and which had been wiped by bibulous paper, in a dry plate-glass tube over mercury; a considerable elevation of temperature was required before any gaseous matter was emitted, but the heat was raised till gas was rapidly formed, and the whole of the amalgam expelled from the tube: in cooling, the mercury rose very quickly in it, so that a great part of the gaseous matter had been either mercury, or water, in vapour, or something which the mercury had absorbed in cooling. The small quantity, which was permanent, did not equal one half the volume of the amalgam.

The gas examined.

On the idea that this gas might be a compound of hidrogen and nitrogen in the state of deoxigenation, I mixed a small quantity of oxigen gas with it, but no change of volume took place; I then exposed it to naphtha, when one half of it was absorbed, which, by the effect the naphtha produced upon turmeric, must have been ammonia; the remaining gas analysed was found to consist of the oxigen that had been introduced, and of hidrogen and nitrogen to each other in the proportion of nearly four to one.

Here ammonia apparently produced without oxigen.

At first I was perplexed by this result, which seemed to prove the production of ammonia, independent of the presence of any substance, which could furnish oxigen to it, and to show that its amalgamation was merely owing to its being freed from water, and combined with hidrogen: but a satisfactory solution of the difficulty soon offered itself.

The difficulty solved.

Exposing the triple amalgam procured from ammonia by potassium to a concentrated solution of ammonia, I found, that it had very little action upon it, and introducing the amalgam moistened by it into a glass tube, it had nearly the same permanency as the amalgam which had been wiped before it was introduced, a little hidrogen only being evolved; but on heating the tube gaseous matter was rapidly generated, which proved to consist of two thirds ammonia, and one third hidrogen.

In the instance in which the amalgam had been wiped, a small quantity of solution of ammonia, and perhaps of potash, must have adhered to it; and though the amalgam does not act upon this powerfully at common temperatures, yet when the water is raised in vapour, it tends to oxigenate both

both the basis of ammonia, and potassium, and hence hidrogen is evolved, and volatile alkali produced.

Distilled in the vapour of naphtha.

I distilled an amalgam procured by potassium from ammonia, in a tube filled with the vapour of naphtha, and hermetically sealed, in the same manner as in the experiments for obtaining the metals of the earths, but in this case I procured ammonia, hidrogen, and nitrogen only, and pure mercury; and the residuum was potassium, which acted powerfully on the glass tube.

Nothing condensible produced but mercury, even when cooled by ice.

In another experiment of the same kind, I kept one part of the tube cool by ice, at the same time the other part was strongly heated, but nothing condensible except mercury was produced, and the elastic products were the same as in the former instance.

Ammonia in the state of gas would not amalgamate.

I endeavoured to procure an amalgam from ammonia, to which no moisture could be supposed to adhere, by heating an amalgam of potassium in ammoniacal gas. The amalgam became covered with a film of potash, but it did not enlarge in its dimensions, and a considerable quantity of nonabsorbable gas, which was found to consist of five parts of hidrogen, and one of nitrogen, was produced. The amalgam after this operation did not emit ammonia by exposure to air, hence it seems probable, that for the deoxigenation of ammonia, and the combination of its basis with mercury, the alkali must be in the nascent state, or at least in that condensed form in which it exists in ammoniacal salts, or solutions.

VI. *Some Considerations of general Theory, connected with the Metallization of the Alkalis and the Earths.*

Properties of the amalgam from ammonia extraordinary.

The more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about $\frac{1}{12000}$ part of its weight of new matter is rendered a solid, yet has its specific gravity diminished from 13·5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

It is scarcely possible to conceive, that a substance, which forms with mercury so perfect an amalgam, should not be metallic

metallic in its own nature*; and on this idea to assist the discussion concerning it, it may be conveniently termed ammonium.

But on what do the metallic properties of ammonium depend?

Are hidrogen and nitrogen both metals in the aeriform state, at the usual temperatures of the atmosphere, bodies of the same character as zinc and quicksilver would be in the heat of ignition? Are hidrogen and nitrogen metals?

Or are these gasses, in their common form, oxides, which become metallized by deoxidation? or oxides?

Or are they simple bodies, not metallic in their own nature, but capable of composing a metal in their deoxigenated, and an alkali in their oxigenated state? or simple bodies composing a metal?

These problems, the second of which was stated by Mr. Cavendish to me, and the last of which belongs to Mr. Berzelius, offer most important objects of investigation.

I have made some experiments in relation to them, but as yet unsuccessfully. I have heated the amalgam of potassium in contact with both hidrogen and nitrogen, but without attaining their metallization; but this fact cannot be considered as decisively for or against any one of these conjectures. Amalgam of potassium treated with hidrogen & nitrogen.

I mentioned in the Bakerian Lecture for 1807, that a modification of a phlogistic chemical theory might be defended on the idea, that the metals and inflammable solids usually called simple, were compounds of the same matter as that existing in hidrogen, with peculiar unknown bases; and that the oxides, alkalis, and acids were compounds of the same bases with water: and that the phænomena presented by the Modification of the phlogistic theory.

* The nature of the compounds of sulphur and phosphorus with mercury favours this opinion; these inflammable bodies by combination impair its metallic properties: cinnabar is a nonconductor, and it would seem from Pelletier's experiments, Ann. de Chimie, vol. xiii, p. 125, that the phosphuret of mercury is not metallic in its characters; charcoal is a conductor, and in plumbago carbon approaches very near to a metal in its characters, so that the metallic nature of steel does not militate against the reasoning in the text. The only facts which I am acquainted with, that do militate against it, are the metallic characters of some of the sulphurets and phosphurets of the imperfect metals.

metals

metals of the fixed alkalis might be explained on this hypothesis.

Less distinct and simple than the received theory.

The same mode of reasoning may be applied to the facts of the metallization of the earths and ammonia, and perhaps with rather stronger evidences in its favour; but still it will be less distinct and simple, than the usually received theory of oxigenation, which I have applied to them.

The general facts of the combustion, and of the action of these new combustible substances upon water, are certainly most easily explained on the hypothesis of Lavoisier; and the only good arguments in favour of a common principle of inflammability flow from some of the novel analogies in electrochemical science.

Is not hidrogen the common element of inflammable bodies?

Assuming the existence of hidrogen in the amalgam of ammonium, its presence in one metallic compound evidently leads to the suspicion of its combination in others. And in the electrical powers of the different species of matter there are circumstances, which extend the idea to combustible substances in general. Oxigen is the only body, which can be supposed to be elementary, attracted by the positive surface in the electrical circuit; and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxigen. Hidrogen is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxigen; may not then the different inflammable bodies, supposed to be simple, contain this as a common element?

Alkalis, earths, and metallic oxides, belong to the same class of bodies.

Should future experiments prove the truth of this hypothesis, still the alkalis, the earths, and the metallic oxides will belong to the same class of bodies. From platina to potassium there is a regular order of gradation as to their physical and chemical properties, and this would probably extend to ammonium, could it be obtained in the fixed form. Platina and gold in specific gravity, degree of oxidability, and other qualities differ more from arsenic, iron, and tin, than these last do from barium and strontium. The phænomena of combustion of all the oxidable metals are precisely analogous. In the same manner as arsenic forms an acid by burning in air, potassium forms an alkali, and calcium an earth; in a manner similar to that in which

osmium

osmium forms a volatile and acrid substance by the absorption of oxigen, does the amalgam of ammonia produce the volatile alkali; and if we suppose that ammonia is metallized, by being combined with hidrogen, and freed from water, the same reasoning will likewise apply to the other metals, with this difference, that the adherence of their phlogiston or hidrogen would be exactly in the inverse ratio of their attraction for oxigen. In platina* it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any of the metals without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalis, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

We are at least one step advanced in our knowledge of the earths and alkalis.

Whatever new lights new discoveries may throw upon this subject, still the facts, that have been advanced, show, that a step nearer at least has been attained towards the true knowledge of the nature of the alkalis and the earths†.

Something

Specific gravities of compounds proportional to the attraction of their parts.

* The common metallic oxides are lighter than their bases, but potash and soda are heavier; this fact may be explained on either theory; the density of a compound will be proportional to the attraction of its parts. Platina, having a weak affinity for oxigen, cannot be supposed to condense it in the same degree as potassium does; or if platina and potassium be both compounds of hidrogen, the hidrogen must be attracted in platina with an energy infinitely greater than in potassium. Sulphuric acid is lighter than sulphur; but phosphoric acid, where there is a stronger affinity, is heavier than phosphorus. The oxide of tin (wood tin) is very little inferior to tin in specific gravity. In this instance the metallic base is comparatively light, and the attraction for oxigen strong; and in a case when the metal is much lighter and the attraction for oxigen stronger, it might be expected a priori, that the oxide would be heavier than the base.

Potassium supposed to be a compound of hidrogen.

† Since the facts in this paper were communicated to the Royal Society, I have seen an account of some very curious experiments of Messrs. Gay Lussac and Thenard, (in Number 148 of the Moniteur, for 1808, which I have just received,) from one of which they have concluded, "that potassium may be a compound of hidrogen and potash."

These gentlemen are said to have heated potassium in ammonia, and found, that the ammonia was absorbed, and that hidrogen gas equal to two thirds of its volume appeared, and that the potassium by this process

The inflammable body

Something has been separated from them which adds to their weight; and whether it be considered as oxigen, or as water

cess had become of a grayish-green colour. By heating this grayish-green substance considerably, two fifths of the ammonia were again emitted, with a quantity of hidrogen and nitrogen corresponding to one fifth more; and by adding water to the mixture, and heating it very strongly again, they obtained the remainder of the ammonia, and nothing but potash was left.

But the supposition gratuitous.

In these complex processes, the phænomena may be as easily explained on the idea of potassium being a simple, as that of its being a compound substance; nor when the facts that have been stated in this paper, and those about to be stated, are considered, can the view of these distinguished chemists, as detailed in the notice referred to, be at all admitted.

Potash, as I have found by numerous experiments, has no affinity for ammonia, for it does not absorb it when heated in it; it is not therefore, allowing their theory, possible to conceive, that a substance having no attraction for potash should repel from it a substance which is intimately combined with it, and which can be separated in no other way.

The phenomena accounted for differently.

A part of the hidrogen evolved in their experiment may be furnished by water contained in the ammonia; but it is scarcely possible, that the whole of it can be derived from this source, for on such an idea the ammonia must contain more than half its weight of water. There is however no evidence, that the whole of the hidrogen may not be furnished by the decomposition of the volatile alkali itself. Potassium in its first degree of oxigenation may have an affinity for nitrogen, or potassium may expel a portion of hidrogen at the moment of its combination with ammonium; and as the whole of the ammonia cannot be regenerated without the presence of water, hidrogen and a little oxigen may be furnished to the remaining elements of the ammonia from the water, and oxigen to the potassium.

Even before the conclusion was formed, that a metallic substance is decomposed in this experiment, it should have been proved, that the nitrogen had not been altered.

Potash cannot form potassium by combination with hidrogen.

That mere potash combined with hidrogen cannot form potassium, is I think shown by an experiment, which I tried, in consequence of the important fact, lately ascertained by Messrs. Gay Lussac and Thenard, of the deoxidation of potash by iron:

Experiment.

An ounce of potash was kept in ignition for some time in an iron tube, ground into a gun barrel in which one ounce and a half of iron turnings were ignited to whiteness; a communication was opened, by withdrawing a wire which closed the tube containing the potash, between that alkali and the metal.

As the potash came into contact with the iron, gaseous matter was developed, which was received in a proper apparatus; and though some of it

water, the inflammable body is less compounded, than the inflammable substance resulting from its combustion. less compounded than

Other

it was lost by passing through the potash into the atmosphere, yet nearly half a cubic foot was preserved, which proved to be hidrogen. In the tube was found two products, one in the quantity of a few grains, containing potassium, combined with a small quantity of iron, and which had sublimated in the operation, and the other a fixed white metallic substance, which consisted of an alloy of iron and potassium.

The first of these substances burnt when thrown upon water; and in its other characters resembled pure potassium, except that its specific gravity was greater, its colour less brilliant, and when it tarnished in the atmosphere, it became of a much deeper colour than pure potassium.

No water present to have furnished hidrogen,

Now potash that has been ignited is the purest form known of this alkali; but on Messrs. Gay Lussac's and Thenard's theory, this potash must contain water, not only sufficient to furnish hidrogen to metallize the alkali, but likewise the quantity disengaged: dry potash then, as it is procured in our experiments, must on this theory be a compound, containing a considerable quantity of matter which can furnish hidrogen; and what would be its form or properties if deprived of this matter we are wholly unable to judge, which brings this question to the general question discussed in the text.

and none in the potash reproduced.

Potassium I find may be produced readily from dry ignited potash in electrical experiments; and the result of the combustion of potassium in oxigen gas is an alkali, so dry that it produces violent heat, and ebullition when water is added to it.

In Messrs. Gay Lussac's and Thenard's experiment on the action of potassium on ammonia, the hidrogen disengaged in the first process, and that existing in the ammonia disengaged in the second proccss, exactly equals the whole quantity contained in the ammonia. But there is no proof of any hidrogen being disengaged from the potassium, for the ammonia lost is not generated nor potash formed, but by the addition of a substance, consisting of oxigen and hidrogen; and as the three bodies concerned in this experiment are potassium, ammonia, and water, the result ought to be potash, ammonia, and a quantity of hidrogen, equal to that evolved by the mere action of water on potassium, which is said to be the case.

Chemical properties of potassium very different from that of a compound of potash and hidrogen.

Even if there were no other proofs, the chemical properties of potassium are so wholly unlike those that might be expected from a compound of potash and hidrogen, that they are almost sufficient to decide the question. Potassium acts upon water with much more energy than potash, and produces much more heat in it; and yet if a compound of hidrogen, the affinity of potash for water must be diminished by its affinity for hidrogen, to say nothing of the quantity of heat, which ought (on the common theory of capcaity for heat) to be carried off by this light inflammable gas.

 Potassium

the uninflammable result of its combustion.

The number of chemical elements might be still reduced.

Other hypotheses might be formed upon the new electrochemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained. Certain electrical states always coincide with certain chemical states of bodies. Thus acids are uniformly negative, alkalis positive, and inflammable substances highly positive; and as I have found, acid matters when positively electrified, and alkaline matters when negatively electrified, seem to lose all their peculiar properties and powers of combination. In those instances the chemical qualities are shown to depend upon the electrical powers; and it is not impossible, that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms*.

I venture

Potassium burns in carbonic acid, and precipitates charcoal from it; whereas hidrogen electrized with carbonic acid converts it into gaseous oxide of carbon.

Potash has a very slight attraction for phosphorus; but potassium has a very strong affinity for it, so as to separate it from hidrogen, and according to Messrs. Gay Lussac and Thenard, with the phenomena of inflammation. Potash has no affinity for arsenic, yet from the experiments of these gentlemen it appears that potassium separates arsenic from arseniated hidrogen; and hidrogen, which is supposed by them to exist in both compounds, can have no affinity for hidrogen, nor can hidrogen in one form be supposed capable of separating arsenic from hidrogen in another form.

Could not the experiment of Messrs. Gay Lussac and Thenard be explained, except on the supposition of the hidrogen being derived from the potassium, it would be a distinct fact in favour of the revival of the theory of phlogiston. It would not prove, however, that potassium is composed of hidrogen and potash, but that it is composed of hidrogen and an unknown basis; and that potash is this basis united to water.

* Phil. Trans. 1807, Part I, p. 23, or our Journal, vol XIX, p. 338.

Difficulties both in the phlogistic and antiphlogistic hypotheses.

The amalgam obtained from ammonia offers difficulties to both the phlogistic and antiphlogistic hypotheses. If we assume the phlogistic hypothesis, then we must assume, that nitrogen, by combining with one fourth of its weight of hidrogen can form an alkali, and by combining with one twelfth more can become metallic. If we reason on the antiphlogistic hypothesis, we must assert, that, though nitrogen has a weaker affinity for oxigen than hidrogen, yet a compound of hidrogen and nitrogen is capable of decomposing water.

The first assumption is however by far the most contradictory to the order of common chemical facts; the last, though it cannot be wholly

removed

I venture to hint at these notions: but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtile powers of matter are but just beginning to be considered; and all general views concerning them must as yet rest upon feeble and imperfect foundations. Theory of chemistry not yet matured.

Whatever be the fate of the speculative part of the inquiry, the facts however will, I hope, admit of many applications, and explain some phænomena in nature.

The metals of the Earth cannot exist at the surface of the globe; but it is very possible, that they may form a part of the interior; and such an assumption would offer a theory for the phænomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat*, and would probably lead to a general hypothesis in geology. Phenomena of volcanoes.

The

removed, is yet lessened by analogies. Thus alloys in general, and inflammable compounds, are more oxidable than the simple substances that compose them. Sulphuret of iron at common temperatures decomposes water with facility; whereas sulphur under the same circumstances has no action on water, and iron a very small one. The compound of phosphorus and hidrogen is more inflammable than either of its constituents.

Should a new theory of the dependence of the chemical forms of matter upon electrical powers be established, the facts belonging to ammonium would admit of a more easy solution. Ammonium might be supposed to be a simple body, which, by combining with different quantities of water, and in different states of electricity, formed nitrogen, ammonia, atmospherical air, nitrous oxide, nitrous gas, and nitric acid. Theory of the dependence of chemical forms on electrical powers.

Water, on this idea, must be supposed a constituent part of all the different gasses; but its electricities in oxigen aud hidrogen would probably be the very reverse of what they have been supposed by Mr. Ritter, and some ingenious English inquirers.

Water positively electrified would be hidrogen, water negatively electrified, oxigen; and as in the physical experiments of temperature, ice, added to certain quantities of steam, by an equilibrium of heat produces water, so in the chemical experiment of the generation of water the positive and negative electricity of oxigen and hidrogen in certain proportions would annihilate each other, and water alone be the result. At all events ammonium, whether simple or compound, must be considered as owing its attraction for oxigen to its highly positive electrical state, which is shown by its powerful determination to the negative surface in the Voltaic circuit.

* Let it be assumed, the metals of the earths and alkalis, in alloy with

common

Meteors.

The luminous appearance of those meteors connected with the fall of stones is one of the extraordinary circumstances of these wonderful phænomena. This effect may be accounted for, by supposing, that the substances, which fall, come into our atmosphere in a metallic state; and that the earths they principally consist of are a result of combustion; but this idea has not the slightest connexion with their origin or cause.

X.

On the supposed universal Distribution of Fossil Coal, in Reply to Mr. B. COOK, *Vol. XXI, page* 292; *and on the Nature and Situations of the extraneous Fossil (Belemnite) analysed by Mr.* J. ACTON, *at page* 305, *under the Denomination of a "Crystal" called a Thunder-pick. In a Letter from Mr.* JOHN FAREY.

To Mr. NICHOLSON.

SIR,

Assertion, that coal may be found in almost all parts of this country,

IT is sincerely to be regretted, when practical and highly useful papers, like that of your correspondent Mr. B. Cook on the advantage of gas lights, in your number for Dec., contain any assertions or speculations, which, being foreign to the profession or pursuits of the writer, are liable to mix errour with so much of practical and useful truth. I was led to these reflections by the following remarks of your correspondent at page 292. "This country produces a vast "quantity of coal, in almost every part where it is properly "sought for;" if gas lights were generally introduced, it might raise the price of coals, "but it certainly would be a "stimulus to men of landed property to seek for it, where

common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water must produce the effect of subterranean fire, and a product of earthy and stony matter analogous to lavas.

"to

"to the present it has been supposed a stranger: it would "therefore, if the demand was so much greater, be found, "I am sure, in greater quantities than at present, as miners "would be induced to seek it every where."

A greater or more common mistake is not often committed, than this which Mr. Cook has fallen into, in supposing, that coals might be any where met with, if sought for; an errour which has occasioned the useless expenditure of hundreds and sometimes of thousands of pounds, in numerous instances, as some in the vicinity of Boxhill in Sussex can testify, on recent experience. A district passing from Somersetshire, through Gloucestershire, Warwickshire, Leicestershire, Nottinghamshire, Derbyshire, Yorkshire, and Durham; and some local districts to the westward and northward of this line, contain numerous and valuable seams or strata of coal; but to the eastward or southward of this line, *no coal ever has or probably ever will be found*, at practicable mining depths, owing to these south-eastern districts being universally covered by great thickness of *upper* strata to any which contain *coal**, and which upper strata seem to cover many of the intervening spaces in the north-western districts of Britain; while in others of these spaces, the coals and their accompanying strata seem wanting, and *lower* strata, from beneath the coal measures, lie exposed.

A mistake of serious moment.

Only coal districts.

The substance called a thunder-pick, the analysis of which is given by Mr. J. Acton at page 305, is not "a crystal," (as Dr. Woodward supposed) but the exuvia of an animal now unknown, called a *belemnite;* which extraneous fossils are frequently found among alluvial matter, on the surface of ploughed lands, mixed with the ruins of the stratum from which they have been dislodged. A stratum in the clay under the Woburn sand produces belemnites in great numbers throughout its whole course, so does a stratum in the great Bath freestone range of hills (see Walcot's Petri-

The thunder-pick not a crystal.

* *Bituminated wood* lodged in white clay, such as occurs at Borry Tracey in Devonshire, has often been confounded by sanguine speculators with fossil coal, to the cruel disappointment of themselves and others.

factions

factions found near Bath, page 34), another in the chalk strata, and perhaps others in the British series of strata.

I am, Sir,

Your humble servant,

JOHN FAREY.

Upper Crown Street, Westminster.

XI.

Account of a British Vegetable Product, that may be Substituted for Coffee. In a Letter from Mr. WILLIAM SKRIMSHIRE, *Jun.*

To Mr. NICHOLSON.

SIR,

British substitute for coffee.

IN the first week of October last, I discovered a vegetable product of British growth, which by particular management may prove an excellent substitute for foreign coffee, and immediately made it a subject of communication to the Scientific Society in this place. But as I cannot learn, that the substance in question has ever been applied to a similar purpose by any other person, I conceive the circumstance of sufficient importance, to claim the public attention, and should you coincide with me in this opinion, I shall be happy to have it form an article in your valuable miscellany.

Yours, &c.

W. SKRIMSHIRE, JUN.

British Coffee.

Common yellow water-flag.

The iris pseudacorus, flower de luce, or common yellow water flag, is a plant which grows in great abundance in some marshes, and by the sides of rivers and ditches.

The seeds

The germen, or seed pod, which is here provincially and vulgarly called *old sows*, is well stocked with seeds covered with chesnut coloured husks. These may be readily thrashed from

from the pods when they are ripe, and, if deposited in a dry place, will keep well for a long time.

This beautiful and ornamental plant is so productive of seeds, that I gathered more than a bushel of them in the space of a few yards, by the side of an old river, in this neighbourhood. Very plentiful.

The seeds of this plant, being roasted in the same manner as coffee is treated, very much resemble it in colour and flavour; but have something more of a saccharine odour approaching to that of the extract of liquorice. However, when carefully prepared, they possess much more of the *aroma* of coffee than is to be found in any of the leguminous and gramineous seeds, that have been treated in the same manner. When roasted as coffee superior to any other substitute.

The government duty upon coffee having been lately taken off, this new product cannot at present be brought to market as a lucrative article of commerce; yet I trust these observations may prove of considerable advantage to those persons, in whose vicinity the iris pseudacorus abounds.

Some persons may perhaps object to the use of these seeds as here recommended, on account of the yellow water flag being a medical plant, possessing so violent a purgative effect upon the human frame, as to render its administration extremely unsafe. I readily grant, that the fresh root is a very drastic cathartic; but I assert, that the other parts of the plant do not possess the same virtues as the root; and that even the root itself, when perfectly dried, is one of the most powerful astringents, that this country produces, and is probably one of the most effectual remedies of this class, that we can employ to remove a diarrhœa, or too great a laxity of the bowels. Besides, I can speak positively from my own experience, that the coffee from the seeds of the yellow water flag is very wholesome and nutritious, in the proportion of half an ounce, or an ounce, to a pint of boiling water. Not cathartic. Even the root, when dried, an astringent. Used by the author.

And as far as a few experiments enable me to form an opinion, I expect, that this product will be found to possess most of the chemical as well as physical properties of the foreign coffee. Has the chemical properties of coffee.

The phenomena which occur in roasting the seeds are

so very similar to what take place in foreign coffee during the same process, that I shall presently relate them.

The seeds described.

The seeds of the iris pseudacorus when ripe, fresh gathered, and freed from the husks, are of a dirty brown colour, semitransparent, and tough like horn. They have, if I may be allowed the expression, a leguminous taste. Their form is various, some are circular and thin, others wedge shaped, while others again are conical, resembling minute bulbous roots. They are between three and four lines in breadth, never more than four, and they are seldom more than two lines in thickness, but generally much thinner.

Beside the arillus, which merely covers the crown of the seed, it is closely enveloped by a very thin brown epidermis, which firmly adheres to the rugous surface of the seed, giving it the appearance of very fine shagreen. When this covering is removed, the seed itself is of a yellowish colour. Under the microscope this epidermis appears to consist of a congeries of papillæ distilling an oil from the surface of the seed underneath *.

Effects of roasting.

When the seeds are exposed to heat upon an iron plate, in order to roast them, they at first crackle and are covered with minute blisters, they change to a reddish brown colour, and are rendered opake; they next become dark brown, and almost black, by the carbonization of the epidermis; they now sweat, or appear oily, emit a dense smoke, and acquire the *aroma* of coffee. If they be taken from the fire at this stage of the process, and wrapped in unsized paper, it absorbs the oil, and different parts of it thereby become transparent.

In this state the epidermis, though carbonized, does not easily separate from the seed, but adheres to its oleaginous surface, giving it a very dirty appearance. But if rubbed in a cotton or woollen cloth, or tossed to and fro in a bag but partly filled with them, they may be freed from this carbonaceous matter, and will thus receive a polish that will enable them to bear handling without staining the fingers.

* The arillus, the epidermis, and even very thin cuttings of the seed itself are exceeding beautiful microscopic objects.

If

If the process of roasting be continued longer, the smoke increases in quantity and density, acquiring a very penetrating empyreumatic odour, the seeds become black from carbonization, and the aroma is entirely destroyed. Over-roasting.

The two great inconveniences in performing this operation with the greatest success are, 1st, the shape of the seeds, which occasions an inequality in the roasting, unless they be continually stirred during the process: 2dly, the tough consistence of the seeds, which renders it necessary to conduct the operation very slowly, for if the fire be too violent, the oil will be burnt and communicate a nauseous flavour to the coffee; and if the seeds be not sufficiently dried and hardened by the continuance of the heat, they will remain too tough for the mill to be able to grind them. Precautions to be observed.

In short, the whole art of roasting them consists in being able to continue the heat long enough to render the seeds of a dark brown colour, perfectly opake, and sufficiently brittle to be readily ground by the mill, without allowing it to be so fierce as to carbonize the oil, which exudes from them.

These very directions are as precisely applicable, and as necessary to be attended to, in roasting foreign coffee, as in preparing the seeds of the iris pseudacorus. These precautions requisite with coffee.

I have preserved the *aroma* of this British coffee in the greatest perfection, by roasting the seeds in the husks; and could a method be contrived for separating the roasted seeds from the carbonized husks, which may easily be done, I have no doubt but this would be by far the best method of conducting the process.

P. S. I hope the idea of presenting the public with a general Index to the Philosophical Journal is not wholly given up. The work itself is now so considerable a depository of knowledge and multifarious information, that it is become a work of daily reference, and the public have a right to expect a general index. So far from the sale of the work being diminished by such a measure, I should think it would be increased by the adoption of a fresh series.

W. S.

XII.

XII.

Account of some ferruginous Rocks serving as Substitutes for Emery. By Mr. Blavier, *Mine Engineer* *.

Corundum generally considered an essential part of emery.

NATURALISTS are agreed in confining the name of emery to a rock containing corundum; but if we were to comprise under this general term every matter capable like the true emery of giving the highest polish and lustre to metals, marbles, granites, and other substances necessary or useful in the arts, we might admit into this class the micaceous iron ore, which occurs in the hollows and on the summit of the granitic table-land between the left bank of the Aveyron and the Viaur.

An iron ore answers the same purposes.

Where found.

This substance is found chiefly, and in the greatest abundance, at the bottom of the mountain of Rodez. Its colour is sometimes gray, at others of a deeper or lighter red, but in either case its fracture is steel-grained. It occurs thus on the banks of the brook of Briane, and at a little distance from Boutonnet, in the commune and arrondissement of Rodez. These ferruginous rocks exist in the hollows in rounded nodules, and in masses, the weight of which sometimes amounts to upward of 5 myriagr. [about 1 cwt.]. May we not suppose, that these nodules are nothing but fragments separated from the veins, that appear of different thicknesses through the quartzose schists deeply tinged with iron, that form the higher hills? This situation is at present well known along the Briane, and it agrees perfectly with the different points, where this mineral has been turned up by the plough. Thus similar blocks are found on the summit of the table-land, particularly in the domain of Puech, and at the side of the monastery, directly south and opposite to the mountain of Rodez. The same ferruginous rock exists in separate and more or less bulky fragments on the back of the hill, that forms the separation between the calcareous band of St. Radegonda and the schistous ground that continues parallel with the left bank of the Aveyron as far as the granitic hill of Levezon. On descending the

* Journal des Mines, No. 111, p. 201.

north-east

north-east declivities of this hill, toward the coalpit of Sensac, we meet with this ferruginous ore in the greatest abundance, and always with the reddish colour of wine lees. Farther researches will lead us to the discovery of its bed, the correspondence of which with the mine of Boutonnet will easily be established, since the narrow flat of Saint Radegonda is the only space that separates them.

Be this as it may, I can aver, that all the schistous land abounds with this kind of ore; and the working it has already engaged the attention of one of the proprietors of Boutonnet, who has solicited from government a permission to search for this substance, in order to its being used in the arts as a cheap substitute for emery. Begun to be worked.

A stamping mill with three pestles and a few troughs would be sufficient for the establishment of a manufactory, the produce of which would be the more important, because, after having extracted the coarser emery, which constitutes the principal consumption of workers in marble and some other artists, the last deposit of the washing would yield a substance capable of supplying the place of English rouge for the last polish given to metals and even glass. For this nothing more is necessary, than to repeat the washings, till it is brought to a sufficient degree of fineness. A substitute likewise for English rouge.

The situation of Rodez is very favourable for such an undertaking; since the manufactories of arms at Tulles and Saint Etienne would occasion a considerable demand for the article; and on the other hand the statuaries of Toulouse and some other neighbouring places would find great advantage in using it. Lastly, it appears, that for polishing looking glasses it might be substituted instead of emery of the first and second quality; and I doubt not but with a little practice and patience the workmen may use it all through the process, when they have learnt to prepare it in a proper manner for grinding and polishing at the same time. The artificial emeries however, which we now know how to compose, prevent our being any longer embarrassed with the difficulty of procuring native emery; and if I recommend the establishment here mentioned, it is particularly on account of the cheapness at which its produce may be obtained, especially by the neighbouring manufacturers. Might be used for polishing mirrors.

XIII.

XIII.

On the Anthophyllite; *by* J. C. Delametherie*.

Anthophyllite found in Norway.

THE anthophyllite described by Schumacher is found at Kongsberg in Norway. It has the appearance of asbestoid, or strahlstein. I have an asbestoid from the Tyrol, that resembles it greatly. Its crystals are prisms, the former of which has not yet been determined. Its colour is a brown green inclining a little to violet. Its specific gravity, according to Karsten, is 3·156†.

Dr. John of Berlin, has analysed this substance, and obtained from a hundred parts,

Its component parts.

Silex	62·66
Alumine	13·33
Oxide of iron	12
Magnesia	4
Lime	3·33
Oxide of magnesia	3·25
	98·57
Loss	1·43
	100.

This analysis evidently approaches near that of the asbestoid; accordingly, I have placed it next the asbestoid in my classification of minerals.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Coal-formation near Durham.

AT the meeting of the society on Saturday the 19th of November, Mr. Mackenzie junior, of Applecross, read a short account of the coal-formation in the vicinity of Durham. From the precise and accurate description communicated by this gentleman, the rocks appear to belong to the oldest coal-formation of Werner. During the course of his observations, he explained what is called the *creep* by miners, and exhibited specimens of the different rocks, and a section of the coal-mine of Kipier, in which both the miners' appellations and the scientific names of the different strata were inserted.

* Journal de Physique, vol. LXIV, p. 356.

† Haüy gives its spec. grav. 3·292, and suspects it to be merely a variety of the Labrador hornblende. T.

At

At the same meeting, Dr. Ogilby of Dublin read the continuation of his description of East Lothian, under the title of Observations on Veins of the newest Floetz-trap of East Lothian. After some preliminary observations on the general geognostic relations of the rocks of East Lothian, and of the precipitation of feldspar in its different states of fineness, from earthy to glassy feldspar, he proceeded to describe the different veins he had an opportunity of examining in this tract of country. These veins he considered as of three different periods of formation; viz. 1. Veins derived from partial formations subsequent to the floetz-trap, which however are not of frequent occurrence; 2. Veins of the different rocks of the formation penetrating the older beds: and, 3. Those of contemporaneous origin. He next enumerated and described, after the manner of Werner, the following veins,—greenstone, jasper, quartz, heavy-spar, and calc-spar; and concluded with several interesting general remarks. Mineralogy of East Lothian.

At this meeting, also, Mr. P. Neill read some observations on the great sea snake of the Northern ocean. He enumerated and read extracts from the different authors, who have mentioned it,—Ramus, Egede, and Pontoppidan. He remarked, that it was placed, by the latter author, between the *mermaid* and the *kraken*, in a chapter which treats on sea monsters; and that, standing in such suspicious company, it had been rejected by naturalists in general as a fabulous creature. He stated however, that, within these few weeks, a vast marine animal, shaped like a snake, and not described in the works of systematic naturalists, had been cast ashore in Orkney. This curious animal, it appears, was stranded in Rothesholm Bay, in the island of Stronsa. Malcolm Laing, Esq., M. P., being in Orkney at the time, communicated the circumstance to his brother Gilbert Laing, Esq., advocate, Edinburgh, on whose property the animal had been stranded, Through this authentic channel Mr. Neill received his information. The creature was dead when it came ashore, and the tail seemed to have been injured and broken by dashing against the rocks. The body measured fifty-five feet in length, and the circumference of the thickest part was equal to the girth of an Orkney poney. The

Great sea-snake.

One lately driven ashore at Orkney.

The head was not larger than that of a seal, and was furnished with two blowholes. From the back, a number of filaments (resembling in texture the substance called silkworm gut, or Indian sea grass) hung down like a mane. On each side of the body were three large fins, shaped like paws, and jointed. Before measures could be taken for securing this rare animal for the inspection of naturalists, a violent tempest unfortunately occurred, and beat the carcase to pieces. Some fragments however have been collected by Mr. Malcolm Laing, and are to be deposited in the Museum of the University of Edinburgh. Mr. Neill concluded with remarking, that no doubt could be entertained, that this was the kind of animal which had served as the prototype of all the wonderful sea-snakes, whose appearance is on record; and that although the unfortunate destruction of the specimen by the storm may probably render it impossible to form a correct generic character on Linnean principles, yet a place (if it should be in an appendix,) could no longer be refused by the most scrupulous naturalists to the serpens marinus magnus of the bishop of Bergen.

Sea unicorn. At the meeting of this Society the 10th of December, the Secretary read a communication from the Rev. John Fleming of Bressay, describing a narwhal, or sea-unicorn, of the sort denominated *le narwal microcephale*, by la Cepède, which had been lately cast ashore alive at Weisdale Sound in Zetland. The description was accompanied with a correct drawing of the animal, which is to be engraved.

Mineralogy of Fassnet. At the same meeting, Dr. Ogilby of Dublin read a paper on the transition greenstone of Fassnet in East Lothian, which beside much valuable mineralogical information, contained a satisfactory answer to the query proposed some time ago by Professor Jameson in regard to the goegnostic relations of the rocks of this tract of country. The descriptions of the individual rocks, and their general and particular geognostic relations, were detailed with ability; and the interest of the whole was increased by acute observations on the mode of examining and discriminating rocks,—a subject of high value, particularly to those who may be employed in examining the mineralogy of a country.

The

The following gentlemen have been elected office-bearers of this society for 1809:— Officers for 1809.

President. R. Jameson, Esq., Prof. Nat. Hist. Edin.

Vice Presidents. Dr. Wright, Dr. Macknight, Dr. Barclay, and Dr. Thomson.

Of the Council. Gen. Dirom, Col. Fullerton, C. S. Menteith, Esq., Dr. Home, Dr. Yule, James Russell, Esq., C. Anderson, Esq., and C. Stewart Esq.

Treasurer. P. Walker, Esq.

Secretary. P. Neill, Esq.

Mr. CARMICHAEL, of Dublin, has in the press a second edition of his Essay on the Effects of Carbonate and other Preparations of Iron upon Cancer, with an Inquiry into the Nature of that Disease. This edition, we understand, is so much enlarged and improved, that it may almost be considered as a new work. Among the additions are a great number of highly interesting cases; a disquisition on the uses of the oxide of iron in the blood; and remarks on such diseases, as depend on its excess or deficiency, or in any way bear a relation to cancer; with an attempt to answer the Queries of the Medical Society established in London for investigating the nature and cure of that complaint. Carmichael on the effect of oxide of iron on cancer, and the uses of oxide of iron in the blood.

Mr. George Singer, has by some recent arrangements considerably improved the original plan of the Scientific Institution, Prince's-street, Cavendish square; at which in future the public lectures are to be assisted by courses of private instruction, and conversations on the various subjects of philosophical inquiry; which are severally illustrated by an extensive and increasing collection of instruments. The attention of the pupils in the ensuing season will be principally directed to subjects of electrical and chemical research; with particular reference to the developement and explanation of the new experiments. A sketch of the plan of this institution, and a prospectus of the lectures, is preparing; and may be shortly obtained at the lecture room. Scientific institution.

London Hospital.

Dr. Buxton's Lectures on the theory and practice of Medicine and on materia medica will be commenced about the 20th. January, 1809. Medical lectures.

METEOROLOGICAL JOURNAL

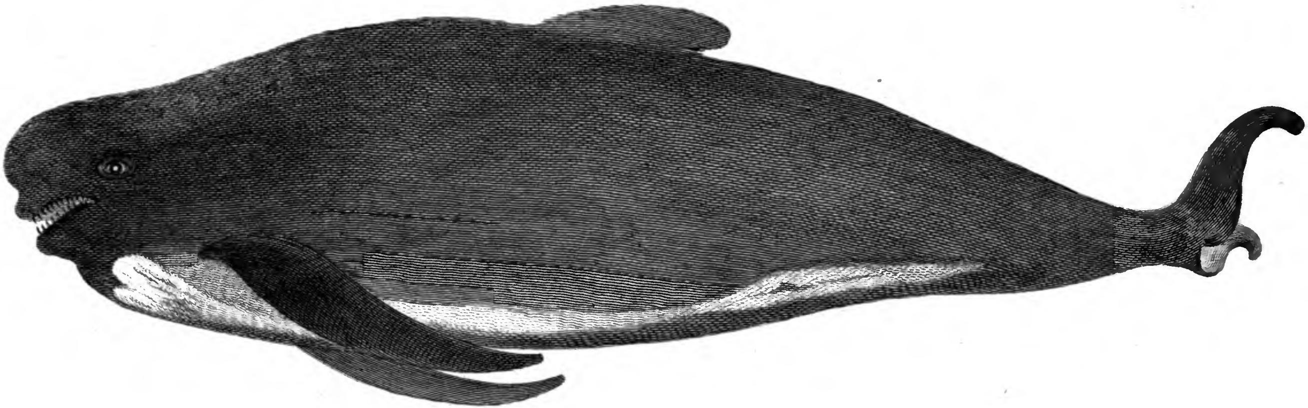
For DECEMBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

NOV. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest.	Lowest.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
26	50	52	54	50	29·98	Cloudy	Rain
27	50	42	54	34	29·50	Fair	Ditto
28	35	36	53	34	29·81	Ditto	Fair
29	36	41	47	40	29·77	Rain	Rain
30	42	41	46	37	29·16	Fair	Fair
DEC.							
1	40	43	47	42	29·36	Ditto*	Ditto
2	41	43	47	41	29·17	Ditto	Ditto
3	42	46	47	42	29·45	Ditto	Ditto
4	41	40	48	36	30·02	Fog	Ditto
5	40	48	49	48	30·27	Mist	Cloudy
6	50	47	52	34	29·97	Fair	Rain
7	35	39	42	32	29·89	Ditto	Fair
8	34	40	41	40	29·95	Ditto	Ditto
9	41	39	44	33	29·86	Fog	Ditto
10	35	40	40	34	30·08	Cloudy	Ditto
11	36	36	42	33	30·25	Fair	Ditto
12	36	41	42	39	30·25	Fog	Cloudy
13	40	35	42	34	30·33	Rain	Ditto
14	34	40	42	33	30·36	Cloudy	Ditto
15	34	33	38	33	30·08	Ditto	Fair*
16	33	33	35	30	30·07	Fair	Ditto
17	32	26	34	22	29·82	Snow†	Snow
18	26	30	32	28	29·62	Fair‡	Fair
19	28	29	33	28	29·65	Cloudy	Snow
20	28	32	33	20	29·69	Fair	Ditto
21	24	31	31	28	29·96	Snow	Ditto
22	28	28	32	26	29·26	Ditto	Ditto
23	27	32	32	27	29·38	Ditto	Ditto
24	28	28	33	26	29·47	Cloudy	Cloudy
25	29	30	30	24	29·56	Ditto	Ditto
26	26	31	32	30	29·49	Ditto	Ditto

* Heavy rain in the night.
† Stars brilliant at 6 P. M., snow at 9, high wind all night.
‡ Snow at 5 P. M. Stars brilliant in the evening. At 12 appearance of change for thaw.

Delphinus. Melas,
A Nondescript Species of the order Cete,
Ninety two of which were driven ashore in Scapay Bay, a few days
before the great Storm in December, 1807.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1809.

ARTICLE I.

Description of a new Species of Whale, Delphinus melas. In a Letter from THOMAS STEWART TRAILL, *M. D.*

To Mr. NICHOLSON.

DEAR SIR,

A Description and correct drawing of an animal never before figured by any naturalist cannot fail of being acceptable to many of your numerous readers. Ninety-two whales of a new species were stranded in Scapay Bay, in Pomona, one of the Orkneys, a few days previous to the great storm in December, 1806. My friend James Watson, Esq. made the enclosed drawing (see Plate III) on the spot, the day after they were driven on shore. New species of whale.

This animal very clearly belongs to the genus *delphinus*, of the class *mammalia*. The only hitherto described species of that genus, which it at all resembles, is the *delphinus orca*, or grampus; but it is distinguished from the grampus by the shape of its snout, the shortness of its dorsal fin, the length and narrowness of its pectoral fins, the form and number of its teeth, and the colour of its belly and breast. Differs from the grampus.

Abundant about the Orkney & Shetland islands.

It abounds in the seas around the Orkney and Shetland Isles. In Mr. Neill's interesting Tour through those islands, we are informed, that 310 of this species were forced on shore in Shetland in 1805. From the imperfect account transmitted to him, this gentleman very properly conjectured them to be a new species.

Description.

Description.—The whole body almost is black, smooth, and shining like oiled silk. The back and sides are jetty black; the breast and belly of a somewhat lighter colour. The general length of the full grown ones is about twenty feet. The body is thick. The dorsal fin does not exceed two feet in length, and is rounded at the extremity. The pectoral fins are from six to eight feet in length, narrow, and tapering to their extremities. The head is obtuse; the upper jaw projects several inches over the lower in a blunt process. It has a single spiracle. The full grown have twenty-two subconoid sharp teeth, a little hooked. Among those stranded in Scapay Bay were many young ones, which, as well as the oldest, wanted teeth. The youngest measured about five feet in length, and were still sucklings. The females had two teats, larger than those of a cow, out of which the milk flowed when they were squeezed.

Habits.

These animals are gregarious, generally swimming in considerable numbers. They frequently enter the bays around the Orkney coast in quest of small fish, which seem to be their food. When one of them takes the ground, the rest surround, and endeavour to assist it: from this circumstance several of them are generally taken at once. I have frequently observed an animal, which I conjecture to be of this species, elevating its dorsal fin and a considerable part of its back above the waves, with a slow tumbling motion for many successive times. They are inoffensive, and rather timid. They are chased on shore not unfrequently by a few yawls. They seem generally to follow one as a leader with blind confidence. I once was in a boat when the attempt was made to drive a shoal of them on shore; but when they had approached very near the land, the foremost turned round with a sudden leap, and the whole rushed past us with great velocity, but carefully avoided the boat. They are extremely fat, and yield a considerable quantity of good oil.

This

This new species may be denominated *delphinus melas*, on the same prineiple on which Gmelin gives the Beluga the name of delph. leucas. The following may serve as its character. DELPHINUS MELAS.—*Corpore crasso, nigro; pinna dorsali una brevi; pinnis pectoralibus longis, angustis; rostro obtuso; maxillo superiore proclinante; dentibus acutis conoideis, parum incurvatis.* Name and character.

THOMAS STEWART TRAILL.

Liverpool, Dec. 17, 1808.

II.

Description of an Apparatus for the Analysis of the Compound Inflammable Gasses by slow Combustion; with Experiments on the Gas from Coal, explaining its Application. By WILLIAM HENRY, *M. D., Vice Pres. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester. Communicated by* H. DAVY, *Esq., Sec. R. S.**

THE aëriform compounds of hidrogen and carbon, which were already entitled to accurate investigation, as objects of scientific research, have derived an additional claim to the attention of the chemist from their application to an important economical purpose, described in a late communication to the Royal Society †. Yet there is, perhaps, no part of chemistry, the investigation of which is beset with greater difficulty, or with more numerous sources of errour; insomuch, that the actual state of the science enables us to attain scarcely more than approximations to the truth, and degrees of probability of greater or less amount. It was the object of the experiments, which are described in the following pages, rather to remove some of the obstacles, which present themselves to a successful inquiry into the nature

Compound inflammable gasses merit accurate examination,

but this liable to many errours,

hereattempted to be removed

* Phil. Trans. for 1808, p. 282.

† See Mr. Murdoch's paper, p. 124; or Journal, vol. XXI, p. 94.

nature of these bodies, than to acquire such facts, as may enable the chemical philosopher to decide the controverted question respecting their composition. Results sufficiently multiplied and precise for this purpose would require a larger appropriation of time, than I have the prospect of being able to bestow; and I can only on the present occasion offer an example of the method, in which it appears to me, that the analysis of this class of substances will be most successfully attempted.

Best method of proceeding.

Vegetable substances.

When a vegetable substance, composed (as may be assumed to simplify the statement) of oxigen, hidrogen, and carbon, united in the form of a ternary compound, is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities, which constituted the triple combination, is destroyed; and the elements, composing it, are united in a new manner. Those, which are disposed to enter into permanently elastic combinations, escape in the state of gas. The carbon, uniting with oxigen, either composes carbonic acid gas, or, stopping short of that degree of oxigenation, which is essential to change it into an acid, is converted into carbonic oxide. The hidrogen, combining with a portion of carbon, constitutes a binary compound of these two ingredients, forming either what has been called *carburetted hidrogen gas*, or *supercarburetted hidrogen*, better known by the appellation of *olefiant gas.* Toward the close of the process, a portion of simple hidrogen gas is also mingled with the products. Perhaps in no instance is any one of the gasses, which have been enumerated, obtained perfectly pure, by the distillation of a vegetable substance. The aëriform fluids, which are thus generated, are found to be possessed of almost every degree of specific gravity; and to yield, by combustion, extremely different results, according to the temperature at which they have been formed; the stage of the process at which they have been separated; and other modifying circumstances. It becomes an interesting question, whether these gasses, so much diversified in their physical and chemical properties, are mixtures of a few binary compounds, with which chemists are already acquainted; or whether, on the contrary, their elements are capable of uniting in indefinite proportions,

The carbon.

The hidrogen.

The gasses always more or less mixed,

of their elements combined in various proportions.

proportions, and of composing ternary compounds of oxigen, hidrogen, and carbon, or varieties of *oxicarburetted* hidrogen. It would encroach too much on the time of the Royal Society, to enter upon this controversy. And, as neither opinion admits, at present, of demonstrative evidence, I may be permitted, in explaining the following experiments, to assume that theory, which appears to me most probable; viz. that the aëriform products of the distillation of vegetable substances are mixtures of carbonic acid, carbonic oxide, olefiant, carburetted hidrogen, and simple hidrogen gasses; or of two or more of these in various proportions.

Most probably mixtures.

The analysis of these compound gasses has hitherto been attempted solely by their rapid combustion with oxigen gas, in the following manner: a mixture of the inflammable gas with oxigen gas in known proportions is admitted into a Volta's eudiometer; inflamed over mercury by the electric spark; and the diminution ascertained. To the remainder caustic potash or lime water is added, by which it sustains a second diminution of bulk; and the amount of this denotes the quantity of carbonic acid formed by the combustion. The quantity of nitrogen gas, in the oxigen employed, as well as in the residue left by potash, being determined by a fit eudiometrical test, it is easy to infer what quantity of oxigen has been absorbed by the detonation. And as it is proved, that oxigen gas sustains no change of bulk by conversion into carbonic acid, we may conclude, that, after deducting from the volume of oxigen gas expended that of the carbonic acid which has been formed, the remaining number shows how much oxigen has been employed in the saturation of hidrogen. If, for example, 100 measures of carburetted hidrogen consume 200 of oxigen gas, and give 100 of carbonic acid; it follows, that the carbonic acid holds in combination 100 measures of the oxigen gas consumed; and that the remaining hundred have been applied to the saturation of hidrogen. In this estimate it is assumed, that the carbon has acquired, by combustion, the whole of the oxigen necessary for its acidification, and that no part of it existed previously in the state of carbonic oxide, a proposition, in many cases, perhaps, very far from being consistent with

Usual mode of analysis.

One source of errour

may be detected.

with the truth. This, however, admits of being decided by an accurate comparison between the weight of the gasses consumed and that of the products.

Gasses already examined.

For the purpose of obtaining a general approximation to the nature of a combustible gas, it may be sufficient to examine its coincidence with those, the properties of which have been already determined. The following table exhibits the results of the combustion of the few gasses, that appear entitled to be considered as distinct species. They are deduced from the experiments of Mr. Cruikshank and Mr. Dalton.

Kind of Gas.	Sp. Grav. (air = 1000.)	100 measures, Take meas. of oxigen.	Give carbonic acid	Are diminished by firing.
Olefiant - - - - - - -	909	300	200	200
Carbonized hidrogen, from stagnant water - -	600	200	100	200
Carbonic oxide - - - - -	967	45	90	55
Hidrogen gas - - - - -	84	50		154

Inflammability of gasses proportional to the oxigen they consume.

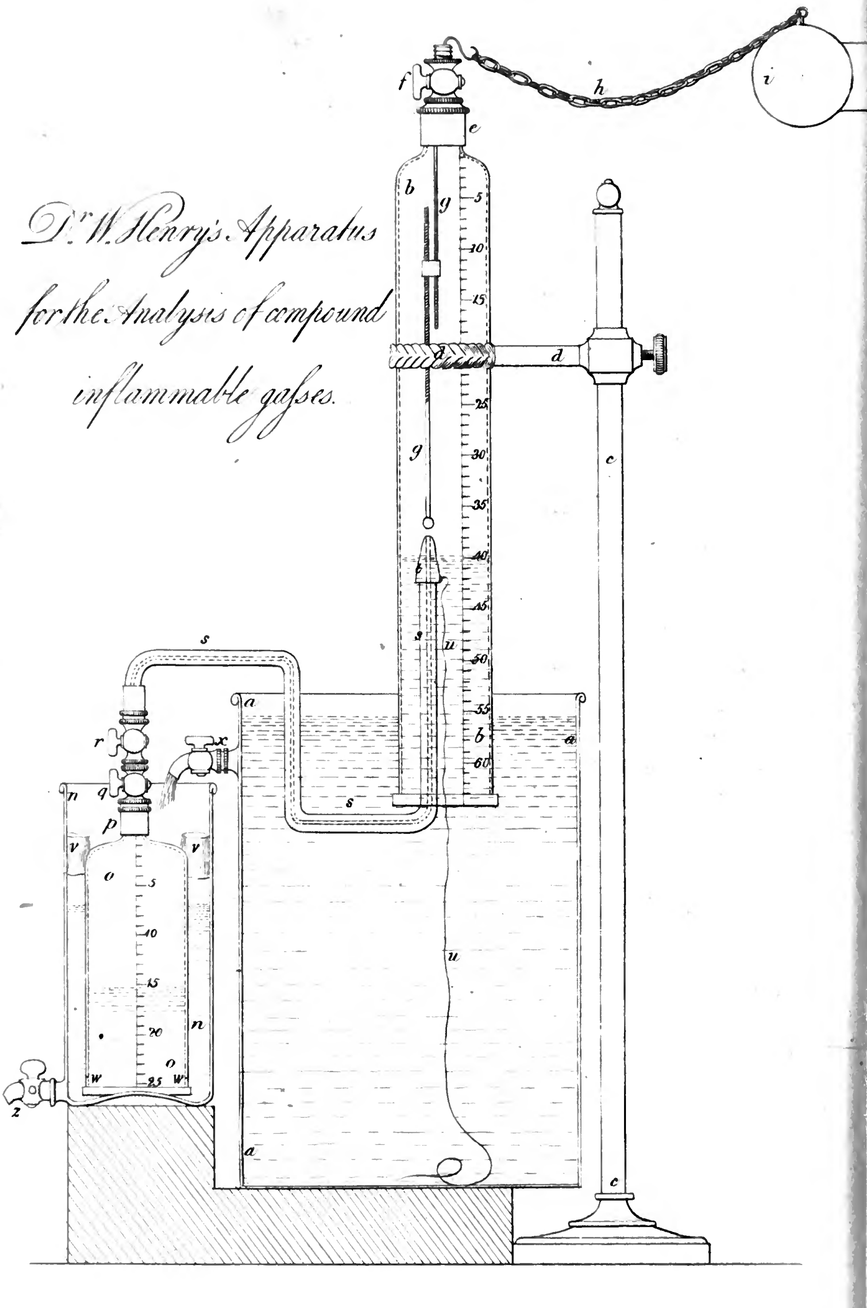
The inflammability of the compound gasses, and their fitness for the purpose of affording light, are directly proportionate to the quantity of oxigen required for their saturation. The olefiant gas, therefore, burns with the greatest brilliancy; carburetted hidrogen gas, though inferior, affords a dense and compact flame; but the carbonic oxide and hidrogen gas are entirely unfit to be employed as the means of artificial illumination.

Results distrusted

In the execution of a series of experiments on the compound combustible gasses, which are described in the 11th volume of Mr. Nicholson's Philosophical Journal, I had reason to be dissatisfied with the above method of effecting their decomposition, and to distrust the results which were obtained. The products of the combustion of the same gas varied considerably in different experiments; and, with respect to some, it was evident, that the full proportion of their carbonaceous ingredient was not oxigenised, in consequence of the precipitation of charcoal in the act of detonation. The quantities, also, that can be submitted in this way

because they varied considerably.

Dr. W. Henry's Apparatus for the Analysis of compound inflammable gasses.



way to experiment, are extremely minute; and the inflammation of highly combustible gasses is attended, as I have more than once experienced, with considerable danger from the bursting of the glass tubes. It was desirable, therefore, to employ a process not liable to these objections; and after many alterations of the apparatus, contrived with this view, I at length fixed upon one, which I shall now proceed to describe.

Experiments not without danger.

The principal parts of the apparatus, are two glass cylinders, or air receivers*, *b b* and *o o*, Pl. IV, of which the larger one is intended to contain oxigen gas, and the smaller one, the inflammable gas submitted to experiment. They are connected by a bent glass tube *s s*, the diameter of which should not be less than $\frac{1}{10}$ of an inch, to the upper extremity of which is cemented an iron burner, *t*, the orifice of which is about $\frac{1}{30}$ of an inch, while to the lower end a socket is fixed, on which may be occasionally screwed the cock *r*. The receiver *o o* is contained in a larger glass jar *n n*, and is closed at the top by a brass cap *p*, and stop cock *q*. The oxigen gas receiver is also closed by a brass cap *e* and cock *f*, the lower orifice of which is tapped internally, for the purpose of receiving a small screw at the end of the copper wire *g*. This wire is in two parts, each of which screws into a movable socket, connecting the two; and, by this contrivance, the wire may be lengthened or shortened at pleasure. To prepare the apparatus for use, the receiver *o o* is partly filled with the combustible gas; and is secured by wedges of cork *v v*, in the jar *n n*, the level of the water in the latter being regulated by opening the cock *x* or *z*. The bent pipe *s s*, with its cock *r*, is screwed upon the top of the receiver, and partly immersed in the water of a pneumatic cistern, *u u*, so that the orifice of the burner may rise a few inches above the surface of the water. The receiver *b b*, detached from the situation in which it is represented in the drawing, is then exhausted by an airpump; and, being filled with oxigen gas, is transferred (its mouth being closed during the act of removal with a piece of leather) to

Apparatus contrived for the analysis described.

Method of preparing it for use.

* I am indebted to Mr. H. Creighton, of Soho, not only for a drawing of the apparatus, but for much valuable assistance in the performance of the experiments.

the

the cistern *a*, and quickly inverted over the burner *t*. By a little practice, this may be done with the admission of very little common air. A transferring vessel is then screwed upon the cock *f*; and a portion of oxigen gas removed for eudiometrical examination. To allow room for the expansion of the oxigen gas, the water is raised by a siphon to a proper height within the receiver *b*, as appears in the drawing.

and of conducting the experiment.

The apparatus being thus disposed, the cock *f* is connected by the chain *h* with the prime conductor of an electrical machine; and a rapid succession of sparks is made to pass between the copper ball at the end of the wire *g*, and the orifice of the burner. The cocks *q* and *r* being now opened, the stream of gas is kindled; and in order to prevent the flame from playing upon the wire, the jar *n n* is moved a little nearer to the cistern *a*, which brings the point of the burner into the axis of the receiver. At the same time, by opening the cock *x*, water flows into the jar *n n*, and finds its way into the receiver, through two small holes *w w* drilled near its mouth.

Calculation of the results.

The combustion continues, until either the whole of the inflammable gas is consumed, or till the cocks *q* and *r* are shut. The wedges *v v* are removed; the receiver *o o* unscrewed; and the bent tube removed from its place. It is at this moment, that the cock *r* is useful, by preventing the escape of the gas from the receiver *b* through the tube *s s*. The upper part of the receiver is cooled by the application of a wet sponge. Without waiting, however, till the gas has attained the temperature of the atmosphere, a very small and sensible thermometer is introduced into it; and the height of the mercury is noted, as soon as it becomes stationary. The volume of the residuary gas is then observed, and is reduced by calculation to the bulk, which it would occupy at 60° of Fahrenheit. Either the whole, or an aliquot part of it is removed by a transferring vessel, screwed upon the cock *f*, to a mercurial cistern, where the proportion of carbonic acid is determined by liquid potash. The proportions of oxigen and nitrogen gasses, in the unabsorbed residue, are learned by agitation with sulphuret of lime, observing the precautions which have been stated by de

Marti.

Marti. The residuary oxigen being deducted from the quantity at the outset of the experiment shows how much oxigen has been expended in the combustion of the inflammable gas. It is scarcely necessary to observe, that the gasses are carefully reduced, at each stage of the operation, to a mean temperature and pressure, (60° of the thermometer, and 30 inches of the barometer)*.

Source of error.

The process of combustion, as thus stated in general terms, appears sufficiently simple. It is often, however, rendered complicated by the imperfect combustion of the inflammable gas, a part of which escapes through the orifice of the burner, either wholly unaltered, or only partially burned. As this portion is not absorbed by sulphuret of lime, it gives a fallacious appearance of an actual addition of nitrogen to the oxigen gas remaining in the receiver *b*. I am unacquainted with any method of entirely obviating this difficulty; but its amount may be diminished by an attention to certain precautions. With this view, the pressure upon the gas, contained in the receiver *o o*, should, on first opening the cocks *q* and *r*, be no more than is sufficient for its gentle expulsion through the tube *s s*. When, however, the stream is once kindled, the larger the flame, and the more active the combustion, within certain limits, the more completely is the gas consumed. It is necessary, also, to stop the combustion, before it is rendered languid by the admixture of carbonic acid with the gas in the receiver *b*, and by the diminished purity of the oxigen gas. If this be not attended to, a large proportion of the inflammable gas, toward the close of the process, makes its escape unaltered into the receiver *b*. In general I have found, that, setting out with oxigen gas of equal purity, the more combustible the inflammable gas submitted to experiment, the more complete is its decomposition by slow combustion. The apparatus, therefore, is better adapted to the analysis of olefiant gas, of carburetted hidrogen gas, or of mixtures of these two, than of carbonic oxide, or any gas of which this oxide forms a large proportion.

Precautions against it.

The more combustible the gas the more complete the decomposition.

* The rules observed in these calculations are stated in my Epitome of Chemistry, 5th edition, p. 441.

The

Method of ascertaining the residual hidrogen.

The inflammable gas, which has found its way into the receiver *b*, is always present in too minute a quantity to compose, with the residuary oxigen, after the removal of the carbonic acid, a mixture capable of being inflamed by the electric spark. To ascertain its precise quantity, it is necessary to have recourse to another operation. After trying, eudiometrically, the quality of an aliquot part of the gas in the receiver *b*, let a similar aliquot part be deprived of its carbonic acid, and then mixed with a portion of pure hidrogen gas, not exceeding one third or one fourth the estimated bulk of the oxigen which it contains. Detonate the mixture, and observe the amount of the diminution after the explosion; the products of the combustion; and the quantity of oxigen gas consumed. After subtracting, from the total expenditure of oxigen, half the bulk of the added hidrogen gas, the remaining number shows how much oxigen has been absorbed by the combustible gas contained in the residue. By the rule of proportion, it may be determined, how much carbonic acid would have been produced, by the oxigenation of the whole of the combustible gas, and what quantity of oxigen it would have saturated.

Objection. Absorption of part of the carbonic acid by the water.

The most obvious objection to this method of analyzing the compound gasses is, that the real proportion of the products, resulting from their combustion, may perhaps be disguised, in consequence of the absorption of a part of the carbonic acid by the water, over which the experiment is made. By frequent trials, however, I find that this is a source of errour too trivial to be deserving of consideration; and that the proportion of carbonic acid, thus generated, exceeds what is composed by the rapid combustion of the same gas over mercury. When the operator has acquired sufficient dexterity, the interval of time between the completion of the combustion and the admeasurement of the residue is too small, to allow an absorption to any notable amount. It must be observed, also, that the carbonic acid constitutes only a small part of the residue; and is, for this reason, very little acted on by water, conformable to a principle which I have explained in the Philosophical Transactions for 1803, p. 274*. I believe, therefore, that, with an

This trifling.

* Journal, vol. V, p. 233.

attention

attention to those observances, which are required in all delicate experiments on gasses, and to the changing circumstances of temperature and pressure, this apparatus is fully adequate to the purpose for which it is intended. It will be easy, however, for those, who have the command of a sufficient quantity of mercury, to adapt the apparatus to this fluid. As an exemplification of the method of using it, in the simplest possible case, I shall state the results of the combustion of hidrogen gas.

Results of the combustion of hidrogen gas.

At the outset of the experiment, there was contained in the receiver *o o* a quantity of hidrogen gas, equal, when reduced to a mean temperature and pressure, to

	15·8 cubic inches.
Of these, there remained unconsumed	2·5
Hidrogen gas burned	13·3

In the receiver *b* were 49 cubic inches of oxigen gas, consisting of

consisting of	33·5 oxigen,	15·5 nitrogen
At the close of the experiment, there remained, in *b*, 43·5 *c. i.* composed of	27·25	16·25
Cubic inches of oxigen gas consumed	6·25	

But estimating from the first diminution (viz. 49—43·5) only 5·5 cubic inches of oxigen would appear to have been absorbed: and the nitrogen gas, by eudiometrical experiments, would seem to have been increased 0·75 of an inch. As the hidrogen gas, however, had been prepared from zinc and sulphuric acid with extreme caution, and did not contain an appreciable quantity of common air, no such addition of nitrogen could have taken place. The apparent increase, then, may be fairly imputed to the escape of 0·75 of an inch of hidrogen gas, which are to be deducted from the 13·3 cubic inches at the outset of the experiment; and hence the real quantity consumed will be 13·3 — 0·75 = 12·55. The true consumption, also, of oxigen gas was 5·50 + 0·75 = 6·25, or pretty exactly, as it ought to be, half the bulk of the hidrogen, which was actually burned.

An

Experiments on olefiant gas.

An example of the analysis of a highly combustible species of elastic fluid is furnished by the following experiments on the olefiant gas, obtained from alcohol and sulphuric acid. Of this gas 100 cubic inches, at a mean of the barometer and thermometer, were equal to 30 troy grains; hence its specific gravity was 967.

In the receiver *o o*, were contained of this gas	6·3 cub. in.
Residue	2
Gas consumed	4·3

In the receiver *b*, were 43·4 inches of oxigen gas. After the combustion, there remained 38·2 cubic inches of mixed gasses, of which 8·6 were carbonic acid. None of the inflammable gas, which passed through the bent tube, had escaped being burned, for the quantity of gas in *b*, not absorbable by sulphuret of lime, so far from having been increased, was found to have sustained a trifling diminution. The oxigen gas, which was consumed, amounted to 13·8 cubic inches. Reducing these results to centesimal proportion, 100 cubic inches of this gas would give 200 of carbonic acid, and absorb 325 of oxigen gas. This experiment agrees with Mr. Dalton's, as to the proportion of carbonic acid from the combustion of olefiant gas, but assigns a larger comsumption of oxigen. It may be observed, however, that the specific gravity of the gas, which I employed, exceeded a little the statement of the Dutch chemists, who found its specific gravity to be 909, common air being 1000.

Gasses from vegetable substances.

Having satisfied myself, by repeated experiments, of the accuracy of the results which may be thus obtained, I proceeded to the combustion of the gasses from a variety of vegetable substances, and especially from those which it seemed probable might become economical sources of light. In the present memoir, I shall describe those only, which were made on coal and a few similar substances, reserving the rest for a future communication.

Gas from cannel coal.

Gas from cannel coal.

This was received in two separate portions. Of the first product, 100 cubic inches, corrected to a mean temperature and

and pressure, weighed 24·28 grains. Hence its specific gravity was to that of atmospheric air as 783 to 1000. The second product was much lighter, 100 inches weighing only 10·4 grains, and having, therefore, the specific gravity of 335. The results are comprehended in the following table. The carbonic acid, stated to have been generated by the second combustion, was formed by adding to an aliquot part of the residue, after the removal of the carbonic acid, a proportion of hidrogen gas; detonating the mixture by the electric spark; and proceeding as already directed. The first two lines contain the minutes of actual experiments; the third and fourth these results reduced to centesimal proportion.

Spec. Grav.	Cubic inches burned.	Oxigen gas consumed.	Carbonic acid generated.	Carb. ac. form'd by 2d combustion.	Ox. consumed by 2d combustion.	Total oxigen consumed.	Total, carbonic acid formed.
783	7·3	16·5	8·3	1·9	0·9	17·4	10·2
335	9·8	9·4	4·8	0	0	9·4	4·8
783	100	222	113·7	26	12	234	139·7
335	100	96	49	0	0	96	49

The first product a mixture of gasses.

The early product of the gas from cannel coal, before being washed with lime water or caustic potash, is a mixture of several different gasses, viz. carbonic acid, sulphuretted hidrogen, olefiant, and a fourth, which is either a gas *sui generis*, or a mixture of carburetted hidrogen, and carbonic oxide. To ascertain the proportion of these gasses in any mixture, is a problem of some difficulty. Sulphuretted hidrogen and olefiant gasses experience, it is well known, an immediate condensation, when mingled with oximuriatic acid gas, and in this way they may be separated from carbonic acid. Again, sulphuretted hidrogen and carbonic acid are absorbed by liquid potash, which has no action on olefiant gas. If, therefore, two equal portions of the gas from coal be mixed with oximuriatic gas, the one in its recent state, the other after being washed with potash, the condensation of the former will be found to exceed that of the washed portion. By the combined use of these agents, we may attain an approximation, at least, to the proportions in which carbonic acid, olefiant, and sulphuretted hidrogen gas

One sui generis?

Method of ascertaining their proportions.

gas are mingled with the aëriform product of coal. The rule may be stated as follows; to a measured quantity of oximuriatic acid gas, contained in a graduated tube, add twice its bulk of the recent coal gas, and at the expiration of one or two minutes observe the diminution which has taken place. Wash an equal quantity with caustic potash; note the loss; and submit the residue to the action of oximuriatic acid as before. The second diminution, thus effected by oximuriatic gas, divided by 2·2, gives the proportion of olefiant gas. Deduct this absorption from the first, and, dividing the remainder by 1·8, we obtain the quantity of sulphuretted hidrogen. Lastly, to know the quantity of carbonic acid gas, subtract, from the diminution effected by potash, the amount of the sulphuretted hidrogen gas. An example, taken from actual experiment, will best explain the application of this rule.

General rule.

One hundred measures of the first product of gas from cannel coal lost, by agitation with liquid potash, 9·7 measures. The remainder, being mingled with one fourth its bulk of oximuriatic acid gas, the mixture lost 10·4 measures. This diminution, 10·4, divided by 2·2, gives 4·9 for the proportion of olefiant gas. But 100 measures of the unwashed gas sustained, by admixture with oximuriatic acid, a diminution of 20 measures. Now, deducting, from this diminution, that occasioned by the condensation of olefiant gas (viz. 20 — 10·4), there remain 9·6, which, divided by 1·8, give 5·3 for the proportion of sulphuretted hidrogen gas. And the diminution by potash (=9·7) — 5·3 gives 4·4 for the proportion of carbonic acid gas. Hence 100 measures of the first product of gas from cannel coal contain,

Practical application of it.

First gas from cannel coal,

1. Of inflammable gas, not affected by the foregoing agents	85·4
2. Of sulphuretted hidrogen gas	5·3
3. Of olefiant gas	4·9
4. Of carbonic acid gas	4·4
	100

The

The proportion of common air, in the foregoing specimen of gas, and in all cases when care was taken to exclude it, was too small to deserve being taken into the account, not appearing, by the test of nitrous gas, to exceed 1 per cent. Very little common air.

The following table exhibits the composition of gas from various kinds of coal. In the last column, under the term inflammable gas, is comprehended that portion, which is neither suddenly condensed by oximuriatic acid gas, nor absorbed by potash. A name more descriptive cannot be applied to it, because it varies essentially in different cases, and the proportion of its components is still matter of doubt. Composition of, gas from different coals, and bitumens.

TABLE I.

Kind of coal.	No. of the product	100 measures consisted of Sulph. hid.	Carb. acid.	Olefiant.	Inflam.
Wigan cannel	1	5·3	4·4	4·9	85·4
	2	0.	1·8	0	98·2
Wednesbury, Staffordsh.	1	4·9	3·4	0	91·7
	2	0·	2·8	0	97·2
Newcastle on Tyne	1	2·9	2·8	2·7	91·6
	2	2·2	1·7	0	96·1
Newcastle, Staffordshire	1	3	2·7	0	94·3
	2	1·4	2	0	96·6
	3	0	1·4	0	98·6
Middleton, near Leeds	1	3	2·	1	94
	2	1·4	1·7	0	96·9
	3	0	2	0	98·
Black Mine, near Manchester	1	3·3	3·2	2·5	91
	2	2	1·1	0	96·9
	3	2	1·	0	97
	4	0·5	1·2	0	98·3
	5	0	1·2	0	98·8
	6	0	0	0	100
Merthyr, Glamorgansh.	1	1	1·7	0	97·3
	2	0	1·7	0	98·3
	3	0	1·6	0	98·4
	4	0	1·5	0	98·5
	5	0	1·	0	99
	6	0	0	0	100
Native coal tar	-	13	6	15	66
Caoutchouc	-	0	4·9	17	78·1

After

After separating the sulphuretted hidrogen and carbonic acid gasses by agitation with liquid potash, the residue, consisting of the inflammable gas mixed with the proportion of olefiant gas produced along with it, was submitted to combustion. The following table shows the average results of a number of these experiments.

Results of the combustion of the inflammable gas.

TABLE II.

Kind of coal.	No of the product	Weight of 100 cubic inches (Ther. 60° Bar. 30°.)	Spec. grav. (Air 1000.)	100 cubic inches consume oxig. gas	100 cubic inches give carbonic acid.
		Grs.			
Wigan cannel ··	1	24·28	783	234	139·7
	2	10·4	335	96	49
Wednesbury coal	1	20·9	674	190	97·5
	2	9·8	316	85	46
Newcastle on Tyne········	1	19·3	622	190	100
	2	9·8	316	86	45
Newcastle, Staffordshire ····	1	19·6	632	195	98
	2	17·7	570	165	80
	3	12·1	390	100	60
Leeds ········	1	20·7	670	190	100
	2	15·1	487	lost by accident.	
	3	9·8	316	85	42
Black Mine, Lancashire ··	1	19·4	627	186	97
	2	15	484	137	65
	3	11·3	364	100	50
	4	10	322	90	47
	5	9·5	307	85	45
	6			80	40
Merthyr ······	1	12	387	117	62
	2	9·5	307	90	47
	3	8	261	75	39
	4	5·9	190	60	31
	5	5·8	187	57	26
	6	5·5	177	50	20
Coal tar ········	-	24·2	780	233	150
Caoutchouc ······	-	—	—	204	121

General observations.

An attentive examination of the results, contained in both the tables, suggests the following general remarks.

Olefiant gas.

1. The olefiant gas is a very sparing product of the distillation of pit coal. It is found only in the first portions, and even of these it does not compose more than 5 per cent. Its

Its quantity, however, is very much influenced by the temperature employed. This remark, indeed, may be extended to all the aëriform products of coal; insomuch that from equal weights of the same coal it is difficult to obtain by different operations, conducted on a small scale, products which are the same either in quantity or quality. The gas from *Coalbrooke dale* tar, and that from *caoutchouc*, have a larger proportion of olefiant gas, which in them amounts to about one sixth of their bulk.

Result much influenced by the heat employed.

2. Sulphuretted hidrogen gas is, also, most abundantly produced at the early stages of the distillation. Its proportion then varies from 1 to 5 per cent; and towards the close of the process it disappears entirely. It increases the illuminating power of the coal gas; but is by no means a desirable product; since it yields by combustion a gas (the sulphurous acid) which is extremely offensive and irritating to the lungs. By the distillation of coal, more sulphuretted hidrogen is produced, than is discovered among the aëriform products; for a part, uniting with the ammonia, which is generated at the same moment, forms sulphuret of ammonia, a compound which I have found among the condensed products.

Sulphuretted hidrogen.

Sulphuret of ammonia.

3. Carbonic acid gas, like the two preceding ones, appears only at an early stage of the process, and in small proportion, never amounting to 5 per cent. A portion of this gas, also, unites with ammonia, and hence carbonate of ammonia is found in the condensed fluid.

Carbonic acid.

4. The gas from coal undergoes a gradual diminution of specific gravity and combustibility, from the commencement to the close of the process. This is best shown by inspecting the results of the experiments on the *Black-Mine* and *Merthyr* coal gas in Table II, because they were reserved in a greater number of separate portions than usual. The progression would, perhaps, have been more regular, in these as well as in the other instances, if much of the gas had not been allowed to escape, in consequence of the immense quantity which was produced. The specific gravity of the coal gas appears to afford a measure of its fitness for illumination, sufficiently accurate for practical uses; but does not bear an exact correspondence to the chemical

The product gradually diminishes in gravity and combustibility.

Carbonic acid not always in proportion to the oxigen expended.

chemical properties of the gas, as ascertained by combustion. It may be remarked, also by comparing the two last columns of the second table, that the carbonic acid produced does not always bear the same proportion to the oxigen expended. Thus the first product of gas from cannel coal combines with 234 measures of oxigen gas; and gives 139·7 of carbonic acid. But the gas from coal tar, with only an equal consumption of oxigen, yields 150 measures of carbonic acid.

Coal gas not the same exactly with any other combustible gas.

5. The aëriform product of coal does not precisely answer to the characters of any one of the combustible gasses, with which we are acquainted. The first product, however, of the distillation of common pit coal, after being washed with potash, approaches very nearly in its properties to carburetted hidrogen gas. The gasses, which surpass this in specific gravity, are mixtures of carburetted hidrogen with olefiant gas, and perhaps a small proportion of carbonic oxide. The lighter gasses, in addition to carburetted hidrogen, probably contain a variable proportion of hidrogen gas and a small quantity of carbonic oxide. The extreme levity of some of the products, especially of the gas from *Merthyr* coal, cannot be explained on any other supposition.

Product of the combustion of coal gas.

6. The products of the combustion of a cubic foot of coal gas, of medium quality, viz. of the specific gravity 622, (such as the first products from *Newcastle on Tyne* coal) may be stated as follows:

	Grains.
A cubic foot, at a mean of the barometer and thermometer ······························	333·5
By combustion, it yields 817·3 grains of carbonic acid, the carbon in which may be estimated* at··	233·7
Grains of hidrogen in a cubic foot of coal gas····	99·8

But 99·8 grains of hidrogen are equivalent to the saturation of 554·9 grains of oxigen, with which they form 654·7 grains of water. Hence the oxigen consumed ought from calculation to be $817{\cdot}3 - 233{\cdot}7 = 573{\cdot}6 + 554{\cdot}9 = 1128$

And the quantity actually consumed appears by experiment to be ······························	1110·3
Errour············	17·7

* Assuming the carbon to be 28 6 grains in 100 grains of carbonic acid, as is satisfactorily proved by the experiments of Messrs. Allen and Pepys.

The

The difference, in this example, between experiment and calculation is not greater, than, in such delicate processes, may always be expected. A part of the deficiency in the oxigen actually consumed may be ascribed, also, to a small portion of the inflammable gas being already in the state of carbonic oxide.

Without repeating the particulars of a similar calculation made on gas of an inferior quality, I shall annex a comparative statement of the specific gravities and composition of the good and inferior gasses.

Difference between the best and inferior gasses.

Source of the Gas.	Weight of a Cubic Foot.	A Cubic Foot consists of		Oxigen Gas consumed by a Cub. Ft.	Gives	
		Carb	Hidr.		Carb. Acid	Water
Newcastle coal	333·5 gr.	233·7	99·8	1110·3	817·3	621
Ditto, last product	169·3	111·5	57·8	560·	400	384·9

The inferior gas, also, probably contains carbonic oxide; and the quantity of oxigen gas, actually consumed, will be found, on calculation, less than it ought to be, if the carbon were not already combined with a portion of oxigen.

The water estimated.

The quantity of water, which was generated by combustion, was not determined experimentally, but is merely estimated. It must be acknowledged, that the decomposition of the inflammable gasses cannot lead to unquestionable results, until the proportion of water, produced by their combustion, is also accurately ascertained. With the view of effecting this, I have already spent much time, and employed many contrivances, none of which have satisfactorily answered the purpose for which they were intended.

Gas from coal burning without flame or smoke.

7. There appears to be a considerable difference in the specific gravity and combustibility of gas from various specimens of coal, even when taken at similar periods of the distillation. The coal from Merthyr in South Wales, which burns without flame or smoke, yields a gas, which contains, in an equal volume, scarcely half as much combustible matter as the gas from Wigan cannel. This will probably be found to be the case with respect to all coal of similar

 quality,

The more abundant the gas, the more it requires purification.

quality, among which may be reckoned the Kilkenny coal. The most important difference among the varieties of this mineral, connected with their application as sources of light, consists in the quantity of sulphuretted hidrogen gas, which is mixed with their aëriform products; and it unfortunately happens, that the coal, otherwise best adapted to this purpose, yields generally the largest proportion of this offensive gas.

Best mode of purifying it.

The only effectual method of purifying the coal gas from sulphuretted hidrogen, on the large scale of manufacture, will probably be found to consist in agitation with quicklime and water, composing a mixture of the consistence of cream. Simple washing with water by no means effects the complete separation.

Condensible fluids neglected.

In the experiments which were made on the products of the distillation of coal, I purposely neglected the amount and analysis of the condensible fluids, because they cannot be advantageously ascertained by the same operation with the elastic ones. They may also be much better determined on the large scale of manufacture, than by limited experiments. For the same reason I was not solicitous to measure even the aëriform fluids; and on this subject, I believe, more accurate information has been communicated by Mr. Murdoch, than it was in my power to acquire.

Manchester, May 19, 1808.

III.

Account of a new irregularity lately perceived in the apparent Figure of the Planet Saturn. By WILLIAM HERSCHEL, *LL. D. F. R. S**.

Singular figure of Saturn.

THE singular figure of Saturn, of which I have given an account in two papers†, has continued, for several reasons, to claim my attention. When I saw the uncommon flatten-

* Philos. Trans. for 1808, p. 159.

† See our Journal, vol. XIII, p. 4.

ing

ing of the polar regions of this planet in the 40-feet telescope, I ascribed it to the attractive matter in the ring‡, and of its tendency to produce such an effect we can have no doubt; but as another circumstance, which was also noticed, namely, an apparent small flattening of the equatorial parts, cannot be explained on the same principles, I wished to ascertain what physical cause might be assigned for this effect, and with a view to an investigation of this point, I have continued my observations. The position of the ring, at the last appearance of the planet, however, proved to be quite unfavourable for the intended purpose; for the very parts, which I was desirous of inspecting, were covered by the passage of the ring over the disk of the planet in front, or were projected on the ring, where it passed behind the body.

Not wholly explicable by the attraction of the ring.

In my attempts to pursue this object, I perceived a new irregularity in the Saturnian figure, which, I am perfectly assured, had no existence the last time I examined the planet, and the following observations contain an account of it.

Recent change in its figure.

Observations.

June 16, 1807. The two polar regions of Saturn are at present of a very different apparent shape. The northern regions, as in former observations, are flattened; but the southern are more curved or bulged outwards.

The two poles differ.

I asked my son John Herschel, who after me looked at Saturn while I was writing down the above observation, if he perceived, that there was a difference in the curvature of the north and south pole; and if he did, to mark on a slate how it appeared to him. When I examined the slate, I found that he had exactly delineated the appearance I have described.

In a letter to a very intelligent astronomical friend*, who has one of my 7-feet reflectors, I requested the favour of him to examine both the polar regions of Saturn, and to let me

‡ See Phil. Trans. for 1805, p. 276; or Journal, vol. xiii, p. 8.

* Dr. Wilson of Hampstead, late Professor of Astronomy at Glasgow.

me know whether he could perceive any difference in the appearance of their curvature; in answer to which I received, the 23d of June, a letter enclosing a drawing, in which also the southern regions were marked as more protuberant, with a greater falling off close to the irregularity. My friend, with his usual precaution, called this an illusion; and it will be seen by and by, that we shall have no occasion to ascribe this irregularity to a real want of due proportion, or settled figure of the polar regions of Saturn.

June 22, 9h 24′. I see the same curved appearance at the south pole of Saturn, which was observed the 16th.

June 24. The air is very clear, and all the most critical phenomena are very distinctly to be seen; the shadow of the ring towards the south upon the planet; the shadow of the body towards the north-following side upon the ring; the belts upon the body; the division of the two rings; and with the same distinctness, I also see the protuberance of the south pole.

Not a distortion of some particular spot in the polar region.

My seeing this appearance, at present, is a proof, that it is not a physical irregularity or distortion of only some particular spot on the polar regions; for, in that case, it could not have been seen this evening, as from the rotation of the planet on its axis, which is 10h 16′, the space of the polar circle which is now exposed to our view must have been very different from what I saw the 16th and 22d.

Many observations were made afterward, which all confirm the reality of this appearance.

It is probably an illusion,

It is so natural for us to reflect upon the cause of a new phenomenon, that I cannot forbear giving an opinion on this subject. To suppose a real change in the whole zone of the planet, cannot be probable; it seems therefore, that this appearance must be, as my friend calls it, an illusion. But since the reality of this illusion, if I may use the expression, has been ascertained by observation, it is certain, that there must be some extrinsic cause for its appearance; and also that the same cause must not act upon the northern hemisphere. Now the only difference in the circumstances under which the two polar regions of Saturn were seen in the foregoing

going observations is the situation of its ring, which passes before the planet at the south, but behind at the north. The rays of light therefore, which come to the eye from the very small remaining southern zone of the saturnian globe, pass at no great distance by the edge of the ring, while those from the north traverse a space clear of every object that might disturb their course. If therefore we are in the right to ascribe the observed illusion to an approximate interposition of the ring, we have, in the case under consideration, only two known causes, that can modify light so as to turn it out of its course, which are inflection and refraction. The insufficiency of the first to account for the lifting up of the protuberant small segment of the southern regions will not require a proof. The effects of refraction on the contrary are known to be very considerable. Let us therefore examine a few of the particulars of the case. The greatest elevation of the visible segment above the ring did not amount to more than one second and three or four tenths. Then supposing the ring, the edge of which is probably of an elliptical figure, to have a surrounding atmosphere, it will most likely partake of the same form, and the rays which pass over its edge will undergo a double refraction: the first on their entrance into this atmosphere, and the second at their leaving it, and these refractions seem to be sufficient to produce the observed elevation. For should they raise the protuberant appearance only half a second, or even less, the segment could no longer range with the rest of the globe of Saturn, but must assume the appearance of a different curvature or bulge outwards.

owing to the position of the ring between the eye and this region,

the ring having an atmosphere by which the rays of light are refracted.

The refractive power of an atmosphere of the ring has been mentioned in a former paper*, when the smallest satellites of Saturn were seen as it were bisected by the narrow luminous lines under which form the ring appeared when the Earth was nearly in the plane of it; and the phenomenon, of which the particulars have now been described, appears to be a second instance in support of the former.

The refractive power of this atmosphere already shown.

* See Phil. Tans. for 1790, page 7.

IV.

IV.

Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood. By THOMAS YOUNG, *M. D. For. Sec. R. S**.

I. *Of the Friction and Discharge of Fluids running in Pipes, and of the Velocity of Rivers.*

Motion of fluids in flexible elastic tubes.

HAVING lately fixed on the discussion of the nature of inflammation, for the subject of an academical exercise, I found it necessary to examine attentively the mechanical principles of the circulation of the blood, and to investigate minutely and comprehensively the motion of fluids in pipes, as affected by friction, the resistance occasioned by flexure, the laws of the propagation of an impulse through the fluid contained in an elastic tube, the magnitude of a pulsation in different parts of a conical vessel, and the effect of a contraction advancing progressively through the length of a given canal. The physiological application of the results of these inquiries I shall have the honour of laying before the Royal Society at a future time; but I have thought it advisable to communicate, in a separate paper, such conclusions, as may be interesting to some persons, who do not concern themselves with disquisitions of a physiological nature; and I imagine it may be as agreeable to the Society, that they should be submitted at present to their consideration, as that they should be withheld until the time appointed for the delivery of the Croonian Lecture.

Dubuat's calculations agree with experiment.

It has been observed by the late Professor Robison, that the comparison of the Chevalier Dubuat's calculations with his experiments is in all respects extremely satisfactory; that it exhibits a beautiful specimen of the means of expressing the general result of an extensive series of observations in an analytical formula, and that it does honour to the penetration, skill, and address of Mr. Dubuat, and of

* Philos. Trans. for 1808, p. 164.

Mr.

Mr. de St. Honoré, who assisted him in the construction of his expressions. I am by no means disposed to dissent from this encomium; and I agree with Professor Robison, and with all other late authors on hydraulics, in applauding the unusually accurate coincidence between these theorems and the experiments from which they were deduced. But I have already taken the liberty of remarking, in my lecture on the history of hydraulics, that the form of these expressions is by no means so convenient for practice as it might have been rendered; and they are also liable to still greater objections in particular cases, since, when the pipe is either extremely narrow, or extremely long, they become completely erroneous: for notwithstanding Mr. Dubuat seems to be of opinion, that a canal may have a finite inclination, and yet the water contained in it may remain perfectly at rest, and that no force can be sufficient, to make water flow in any finite quantity through a tube less than one twenty-fifth of an inch in diameter; it can scarcely require an argument to show, that he is mistaken in both these respects. It was therefore necessary for my purpose to substitute, for the formulæ of Mr. Dubuat, others of a totally different nature; and I could follow Dubuat in nothing but in his general mode of considering a part of the pressure, or of the height of a given reservoir, as employed in overcoming the friction of the pipe through which the water flows out of it; a principle, which, if not of his original invention, was certainly first reduced by him into a practical form. By comparing the experiments, which he has collected, with some of Gerstner, and some of my own, I have ultimately discovered a formula, which appears to agree fully as well as Dubuat's with the experiments from which his rules were deduced, which accords better with Gerstner's experiments, which extends to all the extreme cases with equal accuracy, which seems to represent more simply the actual operation of the forces concerned, and which is direct in its application to practice, without the necessity of any successive approximations.

The form of the expressions not very convenient for practice.

In some cases erroneous.

A formula discovered, that has advantages over Dubuat's.

I began by examining the velocities of the water discharged through pipes of a given diameter with different degrees

Velocities of water discharg-

ed through pipes with different pressures compared.

degrees of pressure; and I found, that the friction could not be represented by any single power of the velocity, although it frequently approached to the proportion of that power, of which the exponent is 1·8; but that it appeared to consist of two parts, the one varying simply as the velocity, the other as its square. The proportion of these parts to each other must however be considered as different in pipes of different diameters, the first part being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very minute tubes; while the second also becomes greater for each given portion of the internal surface of the pipe, as the diameter is diminished.

Formula.

If we express, in the first place, all the measures in French inches, calling the height employed in overcoming the friction f, the velocity in a second v, the diameter of the pipe d, and its length l, we may make $f = a\frac{l}{d}v^2 + 2\,c\frac{l}{d}v$; for it is obvious, that the friction must be directly as the length of the pipe; and since the pressure is proportional to the area of the section, and the surface producing the friction to its circumference or diameter, the relative magnitude of the friction must also be inversely as the diameter, or nearly so, as Dubuat has justly observed. We shall then find that a must be $\cdot0000001\left(430 + \frac{75}{d} - \frac{1440}{d+12} - \frac{180}{d+\frac{1}{3}}\right)$,

and $c = \cdot0000001\left(\frac{900dd}{dd+1000} + \frac{1}{\sqrt{d}}\left(1050 + \frac{12}{d} + \frac{\cdot9}{dd}\right)\right)$.

Hence the velocity may easily be calculated for any pipe or river with any head.

Hence it is easy to calculate the velocity for any given pipe or river, and with any given head of water. For the height required for producing the velocity, independently of friction, is, according to Dubuat, $\frac{v^2}{478}$, or rather, as it appears from almost all the experiments which I have compared, $\frac{v^2}{550}$; and the whole height h is therefore equal to $f + \frac{v^2}{550}$, or $h = \left(\frac{al}{d} + \frac{1}{550}\right)v^2 + \frac{2cl}{d}v$; and making $b = \frac{1}{al:d + \cdot00182}$,

and

and $e = \frac{bcl}{d}$, $v^2 + 2ev = bh$, whence $v = \sqrt{(bh + e^2)} - e$.

In order to adapt this formula to the case of rivers, we must make l infinite; then b becomes $\frac{d}{al}$ and $bh = \frac{d}{a} \cdot \frac{h}{l}$ $= \frac{ds}{a}$, s being the sine of the inclination, and d four times the hydraulic mean depth; and since e is here $= \frac{c}{a}$, $v = \frac{\sqrt{(ads + cc)} - c}{a}$, and in most rivers, v becomes nearly $\sqrt{(20000\, ds)}$.

In rivers.

In order to show the agreement of these formulæ with the result of observation, I have extracted, as indiscriminately and impartially as possible, forty of the experiments made and collected by Dubuat; I have added to these some of Gerstner's, with a few of my own; and I have compared the results of these experiments with Dubuat's calculations, and with my own formulæ, in separate columns. There are six of Dubuat's experiments, which he has rejected as irregular, apparently without any very sufficient reason, since he has accidentally mentioned, that some of them were made with great care: I have therefore calculated the velocities for these experiments in both ways, and compared the results in a separate table.

Various experiments compared with the formula.

Tabular

Tabular Comparison of Hydraulic Experiments.

Observer	d.	$\frac{1}{s}$	Superf. Veloc.	v.	Dub.	Log. ratio.	Y.	Log. ratio.	a ·1[7] ×	c ·1[7] ×	√(20000ds)
DUBUAT.	262·5	35723	15·96	12·56 ?	10·53	·0776	11·10	·0537	424	952	11·1
	258·5	6413	31·77	26·63 ?	28·76	·0334	28·02	·0221	424	952	28·8
	92·4	21827	9·61	7·01 ?	8·38	·0775	8·14	·0649	415	914	9·3
	75·6	27648	7·27	5·07 ?	6·55	·1112	6·27	·0923	413	887	7·5
	17·6	9288		5·70	5·86	·0120	5·97	·0291	376	465	6·1
	16·4	432		32·52	31·61	·0124	30·67	·0255	374	451	27·6
	11·7	1412		14·17	13·59	·0182	14·05	·0037	360	416	12·2
	9·9	427		22·37	24·37	·0372	24·41	·0379	355	414	21·7
	5·8	212		27·51	27·19	·0051	27·34	·0027	352	466	23·5

Observers.	d.	l.	h.	v.	Dub.	Log. ratio.	Y.	Log. ratio	a ·1[7] ×	c ·1[7] ×
COUPLET.	18	43200	145·08	39·16	40·51	·0148	38·49	·0075	376	469
	5	84240	25·00	5·32	5·29	·0024	5·40	·0065	326	492
			16·75	4·18	4·23	·0103	4·21	·0083		
			5·58	2·01	2·25	·0490	2·01	·0000		
BOSSUT.	2·01	2160	24	24·73	24·08	·0115	24·76	·0006	287	747
			12	16·38	16·10	·0075	16·86	·0125		
		1080	24	35·77	35·10	·0082	35·05	·0089		
		360	24	58·90	58·80	·0007	56·85	·0154		
	1·33	2160	12	12·56	12·75	·0065	13·28	·0242	270	919
		1080	24	28·08	28·21	·0020	28·84	·0116		
		360	24	48·53	49·52	·0088	48·66	·0015		
	1.	600	12	22·28	21·98	·0055	22·83	·0106	259	1062
			4	12·22	11·76	·0167	11·92	·0108		
DUBUAT.		737	23·7	28·67	29·41	·0111	30·11	·0213		
			12·2	19·99	19·95	·0009	20·67	·0145		
			4·2	10·56	10·66	·0041	10·90	·0137		
		117	36	84·95	85·52	·0029	83·12	·0069		
			18	58·31	58·47	·0014	58·41	·0012		
	·24167	36·25	53·25	85·77	85·20	·0029	85·71	·0003	309	2268
			41·25	73·81	73·90	·0005	74·67	·0050		
			20·17	51·96	50·14	·0155	50·87	·0093		
			5·00	23·40	23·19	·0039	23·09	·0058		
			·83	7·58	8·22	·0420	7·22	·0212		
	·1667	36·25	51·25	64·37	64·95	·0031	64·08	·0021	402	2827
			38·75	54·19	55·32	·0090	54·93	·0055		
			15·29	33·38	33·17	·0028	32·67	·0094		
			2·04	10·62	10·49	·0053	9·24	·0604		
	·125	34·17	42·17	45·47	46·21	·0070	45·88	·0039	518	3405
			35·33	41·61	41·71	·0010	41·55	·0006		
			14·58	26·20	25·52	·0114	24·94	·0214		
			2·08	7·32	8·35	·0572	6·98	·0206		
						(Mean ·0178 = L. 1·042)		(Mean ·0169 = L. 1·040)		

GERSTNER,

Observers	d.	l.	h.	v.	Dub.	Log. rat.	Y.	Log. rat.	a.	c.
GERSTNER, at 55·5°F.	·2	63	10.7	24·2	23·9	·006	24·1	·002	349	2533
			7·7	21·0	19·9	·023	19·1	·042		
			4·7	15·8	14·9	·026	13·9	·056		
			1·7	7·5	8·2	·039	6·9	·036		
			·7	2·5	5·0	·301	3·4	·133		
	·133	33	10·7	27·1	23·4	·064	22·5	·081	488	3259
			7·7	23·2	19·4	·077	18·5	·098		
			4·7	15·4	14·6	·024	13·5	·058		
			1·7	5·6	8·1	·160	6·7	·078		
			·7	2·3	4·6	·301	3·4	·169		
	·0674	33	10·7	10·0	8·9	·051	10·1	·004	975	5700
			7·7	7·2	7·4	·012	8·2	·057		
			4·7	4·5	5·6	·095	5·6	·095		
			1·7	1·5	3·1	·316	2·5	·222		
			·7	·5	1·8	·444	1·1	·342		
					(Mean	·129=L. 1·346		·098=L. 1·254)		
Y. at 60°.	$\frac{1}{42}$	8 50	32·4	14·40	o	∞	13·36	·032	2956	13882
	$\frac{1}{163}$	3 42	30·0	·53			·52	·008	13404	452100
		1·17	5·8	·27			·30	·046		
							(Mean	·029		
DUBUAT.	2	255·25	36·35	86 31	84·2	·011	79·7	·035	287	747
	1	24	36·25	122·59	117·8	·018	120·8	·007	259	1063
			27	106·45	101·1	·022	104·1	·010		
			18	84·85	82·2	·013	84·8	·000		
			9	59·25	57·5	·013	59·7	·004		
		4	27·08	118·67	111·5	·027	118·5	·000		
					(Mean	·017=L. 1·041		·009=L. 1·022		

It appears from this comparison, that in the forty experiments extracted from the collection, which served as a basis for Dubuat's calculations, the mean errour of his formula is $\frac{1}{24}$ of the whole velocity, and that of mine $\frac{1}{25}$ only; but if we omit the four experiments, in which the superficial velocity only of a river was observed, and in which I have calculated the mean velocity by Dubuat's rules, the mean errour of the remaining 36 is $\frac{1}{35}$, according to my mode of calculation, and $\frac{1}{37}$ according to Mr. Dubuat's; so that on the whole, the accuracy of the two formulæ may be considered as precisely equal with respect to these experiments. Accuracy of the two formulæ compared.

In

In the six experiments which Dubuat has wholly rejected, the mean errour of his formula is about $\frac{1}{14}$, and that of mine $\frac{1}{15}$. In fifteen of Gerstner's experiments, the mean errour of Dubuat's rule is one third, that of mine one fourth; and in the three experiments which I made with very fine tubes, the errour of my own rules is one fifteenth of the whole, while in such cases Dubuat's formulæ completely fail. I have determined the mean errour by adding together the logarithmic ratios of all the results, and dividing the sum by the number of experiments. It would be useless to seek for a much greater degree of accuracy, unless it were probable, that the errours of the experiments themselves were less than those of the calculations; but if a sufficient number of extremely accurate and frequently repeated experiments could be obtained, it would be very possible, to adapt my formula still more correctly to their results.

In order to facilitate the computation, I have made a table of the coefficients *a* and *c* for the different values of *d*, all the measures being still expressed in French inches.

Table of Coefficients for French Inches.

d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$
00	430	900	15	370	427	·7	249	1278
500	427	943	10	354	414	·6	248	1384
400	426	946	9	350	421	·5	249	1524
300	423	950	8	345	433	·4	257	1717
200	421	951	7	340	440	$\frac{1}{3}$	268	1895
100	416	923	6	335	462	·3	279	2008
90	415	911	5	325	512	$\frac{1}{4}$	303	2225
80	413	896	4	319	540	·2	349	2532
70	410	872	3	305	617	$\frac{1}{6}$	402	2827
60	408	840	2·5	296	687	·15	440	3026
50	406	792	2	288	751	$\frac{1}{7}$	458	3116
40	400	719	1·5	275	866	$\frac{1}{8}$	518	3405
30	393	618	1	259	1063	$\frac{1}{9}$	589	3693
25	387	560	·9	255	1123	·1	646	3985
20	380	492	·8	252	1193			

For

For example, in the last experiment, where d is 1, l 4, and h 27·1, we have $a = \cdot0000259$, $b = \frac{1}{al:d + \cdot00182} = 516$, $c = \cdot0001063$, $e = bcl:d = \cdot22$, and $v = \surd\,(bh + e^2) - e = 118\cdot46$, which agrees with the experiment within $\frac{1}{500}$ of the whole. I had at first employed for a the formula $\frac{430}{1 + 12:d} + \frac{57}{d} + \frac{1}{6dd}$, but I found, that the value, thus determined, became too great when d was about 20, and too small in some other cases. Coulomb's experiments on the friction of fluids, made by means of the torsion of wires, give about ·00014 for the value of c, which agrees as nearly with this table, as any constant number could be expected to do. I have however reason to think, from some experiments communicated to me by Mr. Robertson Buchanan, that the value of a, for pipes about half an inch in diameter, is somewhat too small; my mode of calculation, as well as Dubuat's, giving too great a velocity in such cases.

Remarks on the coefficients.

If any person should be desirous of making use of Dubuat's formula, it would still be a great convenience to begin by determining v according to this method; then, taking $b = \frac{l}{h - v^2 : 478}$, or rather, as Langsdorf makes it, $b = \frac{l}{h - v^2 : 482}$, to proceed in calculating v by the formula $v = 148\cdot5\,(\surd d - \cdot2)\,.\left(\frac{1}{\surd b - \text{H. L.}\surd\,(b + 1\cdot6)} - \cdot001\right)$, since this determination of b will, in general, be far more accurate than the simple expression $b = \frac{l + 45d}{h}$, and the continued repetition of the calculation, with approximate values of v, may thus be avoided. Sometimes, indeed, the values of v found by this repetition will constitute a diverging instead of a converging series, and in such cases, we can only employ a conjectural value of v, intermediate between the two preceding ones.

Assistants to Dubuat's formula.

Having sufficiently examined the accuracy of my formula, I shall now reduce it into English inches, and shall add

The formula reduced to English measure.

add a second table of the coefficients, for assisting the calculation. In this case, a becomes $\cdot 0000001 \left(413 + \frac{75}{d} - \frac{1440}{d + 12\cdot 8} - \frac{180}{d + \cdot 355}\right)$, $c = \cdot 0000001 \left(\frac{900dd}{dd + 1136} + \frac{1}{\surd d}\right.$ $\left.(1085 + \frac{13\cdot 21}{d} + \frac{1\cdot 0563}{dd}\right)$, and $b = \frac{1}{al : d + \cdot 00171}$, e being $\frac{bcl}{d}$, and $v = \surd\ (bh + e^2) - e$, or $= \surd \left(\frac{ds}{a} + \frac{cc}{aa}\right) - \frac{c}{a}$, as before; and in either case the superficial velocity of a river may be found, very nearly, by adding to the mean velocity v its square root, and the velocity at the bottom by subtracting it.

Table of Coefficients, for English Inches.

d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$
00	413	900	15	354	430	·7	243	1322
500	410	944	10	339	413	·6	243	1433
400	409	948	9	336	421	·5	245	1578
300	406	951	8	331	433	·4	254	1779
200	404	951	7	327	449	$\frac{1}{3}$	268	1963
100	399	918	6	322	471	·3	280	2082
90	398	903	5	312	507	$\frac{1}{4}$	305	2307
80	396	885	4	306	556	·2	354	2631
70	393	860	3	292	635	$\frac{1}{6}$	409	2943
60	391	825	2·5	284	694	·15	447	3150
50	389	772	2	277	774	$\frac{1}{7}$	466	3251
40	383	698	1·5	266	894	$\frac{1}{8}$	528	3558
30	377	597	1	251	1099	$\frac{1}{9}$	599	3866
25	371	526	·9	248	1161	·1	657	4183
20	364	482	·8	245	1234			

II. *Of the Resistance occasioned by Flexure in Pipes or Rivers.*

Dubuat's experiments on effect of flexure in a pipe.

Mr. Dubuat has made some experiments on the effect of the flexure of a pipe in retarding the motion of the water flowing through it; but they do not appear to be by any means

means sufficient to authorise the conclusions, which he has drawn from them. He directs the squares of the sines of the angles of flexure to be collected into one sum, which, being multiplied by a certain constant coefficient, and by the square of the velocity, is to show the height required for overcoming the resistance. It is, however, easy to see, that such a rule must be fundamentally erroneous, and its coincidence with some experiments merely accidental, since the results afforded by it must vary according to the method of stating the problem, which is entirely arbitrary. Thus it depended only on Mr. Dubuat to consider a pipe bent to an angle of 144° as consisting of a single flexure, as composed of two flexures of 72° each, or of a much greater number of smaller flexures; although the result of the experiment would only agree with the arbitrary division into two parts, which he has adopted. This difficulty is attached to every mode of computing the effect either from the squares of the sines or from the sines themselves; and the only way of avoiding it is to attend merely to the angle of flexure as expressed in degrees. It is natural to suppose, that the effect of the curvature must increase, as the curvature itself increases, and that the retardation must be inversely proportional to the radius of curvature, or very nearly so; and this supposition is sufficiently confirmed by the experiments which Mr. Dubuat has employed in support of a theory so different. It might be expected, that an equal curvature would create a greater resistance in a larger pipe than in a smaller, since the inequality in the motions of the different parts of the fluid is greater; but this circumstance does not seem to have influenced the results of the experiments made with pipes of an inch and of two inches diameter: there must also be some deviation from the general law in cases of very small pipes having a great curvature, but this deviation cannot be determined without farther experiments. Of the 25 which Dubuat has made, he has rejected 10 as irregular, because they do not agree with his theory: indeed 4 of them, which were made with a much shorter pipe than the rest, differ so manifestly from them, that they cannot be reconciled: but 5 others agree sufficiently, as well as all the rest, with the theory which I have here proposed, supposing the

His rule fundamentally erroneous.

A different theory.

Sufficiently comfirmed by experiments.

the resistance to be as the angular flexure, and to increase besides almost in the same proportion as the radius of curvature diminishes, but more nearly as that power of the radius of which the index is $\frac{7}{8}$. Thus if p be the number of degrees subtended at the centre of flexure, and q the radius of curvature of the axis of the pipe in French inches, we shall have $r = \frac{p v^2}{200000 q}$ nearly, or, more accurately, $r = \frac{\cdot 0000045\, p v^2 q^{\frac{1}{8}}}{q}$. These calculations are compared with the whole of Dubuat's experiments in the following table.

Table of Experiments on the Resistance occasioned by Flexure.

p	q	v^2	r	B.	Y. 1	Y. 2
288	3.22	15030	4·75		6·71	6·98
		11330	3·50		5·06	5·26
		7199	2·33		3·21	3·34
		3510	1·08		1·56	1·62
216		7216	2·49	2·49	2·42	2·52
144			1·50	1·66	1·61	1·67
72			·75	·83	·80	·83
196·5	6·12		1·50	1·66	1·16	1·31
147·4			1·12	1·24	·87	·98
98·3			·75	·83	·58	·65
49·1			·37	·41	·29	·33
112·5	·53		6·00		7·68	6·36
99			5·90		6·74	5·60
288	3·22	3415	1·50	1·57	1·52	1·58
288	3·22	3415	1·50	1·57	1·52	1·58
144			·75	·78	·76	·79
72			·37	·39	·38	·39
196·5	6·12		·75	·78	·55	·62
112·5	·53		1·50		3·63	3·00
720	3·22	5125	5·90	5·90	5·72	5·95
288		3458	1·64	1·59	1·54	1·60
		860	·41	·40	·38	·40
—		821	·39	·38	·37	·38
288	4·10	3448	1·33		1·21	1·30
		7449	2·90		2·59	2·78
294·8	9·9					
360	4·1		8·64		8·08	8·62
112·5	1·1					

In

In the last three experiments, the diameter of the pipe was two inches. The radius of curvature is not ascertained within the tenth of an inch, as Dubuat has not mentioned the thickness of the pipes. The mean errour of his formula in fifteen experiments, and of mine in twenty, is $\frac{1}{25}$ of the whole. Remarks.

III. *Of the Propagation of an Impulse through an Elastic Tube.*

Propagation of an impulse through an elastic tube.

The same reasoning, that is employed for determining the velocity of an impulse, transmitted through an elastic solid or fluid body, is also applicable to the case of an incompressible fluid contained in an elastic pipe; the magnitude of the modulus being properly determined, according to the excess of pressure which any additional tension of the pipe is capable of producing; its height being such, as to produce a tension, which is to any small increase of tension produced by the approach of two sections of the fluid in the pipe, as their distance to its decrement: for in this case the forces concerned are precisely similar to those, which are employed in the transmission of an impulse through a column of air enclosed in a tube, or through an elastic solid. If the nature of the pipe be such, that its elastic force varies as the excess of its circumference or diameter above the natural extent, which is nearly the usual constitution of elastic bodies, it may be shown, that there is a certain finite height which will cause an infinite extension, and that the height of the modulus of elasticity, for each point, is equal to half its height above the base of this imaginary column; which may therefore be called with propriety the modular column of the pipe: consequently the velocity of an impulse will be at every point equal to half of that which is due to the height of the point above the base; and the velocity of an impulse ascending through the pipe being every where half as great as that of a body falling through the corresponding point in the modular column, the whole time of ascent will be precisely twice as great as that of the descent of the falling body; and in the same manner if the pipe be inclined, the motion of the impulse may be compared

Propositions.

pared with that of a body descending or ascending freely along an inclined plane.

Demonstration. These propositions may be thus demonstrated: let a be the diameter of the pipe in its most natural state, and let this diameter be increased to b by the pressure of the column c, the tube being so constituted, that the tension may vary as the force. Then the relative force of the column c is represented by $b\,c$, since its efficacy increases, according to the laws of hydrostatics, in the ratio of the diameter of the tube; and this force must be equal, in a state of equilibrium, to the tension arising from the change from a to b, that is, to $b-a$; consequently, the height c varies as $\frac{b-a}{b}$; and if the tube be enlarged to any diameter x, the corresponding pressure required to distend it will be expressed by a height of the column equal to $\left(1-\frac{a}{x}\right).\frac{bc}{b-a}$ since $\frac{b-a}{b}:c::\frac{x-a}{x}:\left(1-\frac{a}{x}\right)\frac{b\,c}{b-a}$. Now if the diameter be enlarged in such a degree, that the length of a certain portion of its contents may be contracted in the ratio $1:1-r$, r being very small, then the enlargement will be in the ratio $1:1+\frac{r}{2}$, that is, x' will be $\frac{rx}{2}$; but the increment of the force, or of the height, is $\frac{a\,x'}{x\,x}\cdot\frac{b\,c}{b-a}$, which will become $\frac{a\,r}{2x}\cdot\frac{b\,c}{b-a}$. Now in a tube filled with an elastic fluid, the height being h, the force in similar circumstances would be $r\,h$, and if we make $h=\frac{a}{2x}\cdot\frac{b\,c}{b-a}$, the velocity of the propagation of an impulse will be the same in both cases, and will be equal to the velocity of a body which has fallen through the height $\frac{1}{2}h$. Supposing x infinite, the height capable of producing the necessary pressure becomes $\frac{b\,c}{b-a}$, which may be called g, and for every other value of x this height is $\left(1-\frac{a}{x}\right)g$, or $g-\frac{ag}{x}$, or, since h becomes

$a\,g$

$\frac{ag}{2x}$, $g-2h$, so that h is always equal to half the difference between g and the actual height of the column above the given point, or to half the height of the point above the base of the column.

If two values of x, with their corresponding heights, are given, as b and x, corresponding to c and d, and it is required to find a; we have $\frac{b-a}{b} : c :: \frac{x-a}{x} : d$, $dbx-dax = cbx-cba$, and $a = \frac{dbx-cbx}{dx-ch}$, or $\frac{b}{a} = \frac{dx-ch}{dx-cx}$. Thus if the height equivalent to the tension vary in the ratio of any power m of the diameter, so that, n being a small quantity, $x = b(1+n)$ and $d = c(1+mn)$, $\frac{b}{a} =$

$$\frac{bc\left((1+n).(1+mn)-1\right)}{bc\left((1+n).(1+mn-(1+n)\right)} = \frac{mn+n}{mn},$$ since the square of n is evanescent, and $\frac{b}{a} = \frac{m+1}{m}$. For example, if $m=4$, $\frac{b}{a} = \frac{5}{4}$, and if $m=2$, $b:a::3:2$.

IV. *Of the Magnitude of a diverging Pulsation at different Points.*

Magnitude of a diverging pulsation at different points.

The demonstrations of Euler, Lagrange, and Bernoulli, respecting the propagation of sound, have determined, that the velocity of the actual motion of the individual particles of an elastic fluid, when an impulse is transmitted through a conical pipe, or diverges spherically from a centre, varies in the simple inverse ratio of the distance from the vertex or centre, or in the inverse subduplicate ratio of the number of particles affected, as might naturally be inferred from the general law of the preservation of the ascending force or impetus, in all cases of the communication of motion between elastic bodies, or the particles of fluids of any kind. There is also another way of considering the subject, by which a similar conclusion may be formed respecting waves diverging from or converging to a centre. Suppose a straight wave

Waves.

wave to be reflected backwards and forwards in succession, by two vertical surfaces, perpendicular to the direction of its motion; it is evident that in this and every other case of such reflections, the pressure against the opposite surfaces must be equal, otherwise the centre of inertia of the whole system of bodies concerned would be displaced by their mutual actions, which is contrary to the general laws of the properties of the centre of inertia. Now if, instead of one of the surfaces, we substitute two others, converging in a very acute angle, the wave will be elevated higher and higher as it approaches the angle: and if its height be supposed to be every where in the inverse subduplicate ratio of the distance of the converging surfaces, the magnitude of the pressure, reduced to the direction of the motion, will be precisely equal to that of the pressure on the single opposite surface, which will not happen if the elevation vary inversely in the simple ratio of the distance, or in that of any other power than its square root. This mode of considering the subject affords us therefore an additional reason for asserting, that in all transmissions of impulses through elastic bodies, or through gravitating fluids, the intensity of the impulse varies inversely in the subduplicate ratio of the extent of the parts affected at the same time; and the same reasoning may without doubt be applied to the case of an elastic tube.

Intensity of the impulse varies inversely in the subduplicate ratio of the extent affected.

Waves crossing each other.

There is however a very singular exception, in the case of waves crossing each other, to the general law of the preservation of ascending force, which appears to be almost sufficient to set aside the universal application of this law to the motions of fluids. It is confessedly demonstrable, that each of two waves, crossing each other in any direction, will preserve its motion and its elevation with respect to the surface of the fluid affected by the other wave, in the same manner as if that surface were plane: and, when the waves cross each other nearly in the same direction, both the height and the actual velocity of the particles being doubled, it is obvious, that the ascending force or impetus is also doubled, since the bulk of the matter concerned is only halved, while the square of the velocity is quadrupled; and supposing the double wave to be stopped by an obstacle, its magnitude

magnitude, at the moment of the greatest elevation, will be twice as great as that of a single wave in similar circumstances, and the height, as well as the quantity of matter, will be doubled, so that either the actual or the potential height of the centre of gravity of the fluid seems to be essentially altered, whenever such an interference of waves takes place. This difficulty deserves the attentive consideration of those, who shall attempt to investigate either the most refined parts of hydraulics, or the metaphysical principles of the laws of motion.

V. *Of the Effect of a Contraction, advancing through a Canal.*

End of a rectangular horizontal velocity partly filled with water advancing with a given velocity.

If we suppose the end of a rectangular horizontal canal, partly filled with water, to advance with a given velocity, less than that with which a wave naturally moves on the surface of the water, it may be shown, that a certain portion of the water will be carried forwards, with a surface nearly horizontal, and that the extent of this portion will be determined, very nearly, by the difference of the spaces described, in any given time, by a wave, moving on the surface thus elevated, and by the movable end of the canal. The form of the anterior termination of this elevated portion, or wave, may vary, according to the degrees by which the motion may be supposed to have commenced; but whatever this form may be, it will cause an accelerative force, which is sufficient to impart successively to the portions of the fluid, along which it passes, a velocity equal to that of the movable end, so that the elevated surface of the parts in motion may remain nearly horizontal: and this proposition will be the more accurately true, the smaller the velocity of the movable end may be. For, calling this velocity v, the original depth a, the increased depth x, and the velocity of the anterior part of the wave y, we have, on the supposition that the extent of the wave is already become considerable, $x = \frac{a y}{y \mp v}$, taking the negative or positive sign according to the direction of the motion of the end; since the quantity of fluid, which before occupied a length expressed

pressed by y, now occupies the length $y \mp v$; and putting $a \backsim x = z$, $z = \frac{av}{y \mp v}$. The direction of the surface of the margin of the wave is indifferent to the calculation, and it is most convenient to suppose its inclination equal to half a right angle, so that the accelerating force, acting on any thin transverse vertical lamina, may be equal to its weight: then the velocity y must be such, that while the inclined margin of the wave passes by each lamina, the lamina may acquire the velocity v by a force equal to its own weight; consequently the time of its passage must be equal to that in which a body acquires the velocity v, in falling through a height b, corresponding to that velocity: and this time is expressed by $\frac{2b}{v}$; but the space described by the margin of the wave is not exactly z, because the lamina in question has moved horizontally during its acceleration, through a space which must be equal to b; the distance actually described will therefore be $z \pm b$, and we have $\frac{z \pm b}{y} = \frac{2b}{v}$, $z \pm b = \frac{2by}{v}$, $av \pm by - bv = \frac{2byy}{v} \mp 2by$, $y^2 \mp \frac{3}{2}vy = \frac{av^2}{2b} - \frac{v^2}{2}$, $\left(y \mp \frac{3}{4}v\right)^2 = \frac{av^2}{2b} + \frac{v^2}{16}$; but, m being the proper coefficient, $v = m \surd b$, and $v^2 = m^2 b$, $\frac{av^2}{2b} + \frac{v^2}{16} = m^2 \left(\frac{a}{2} + \frac{b}{16}\right)$, $y = m \surd \left(\frac{a}{2} + \frac{b}{16}\right) \pm \frac{3}{4}v$, and $y \mp v = m \surd \left(\frac{a}{2} + \frac{b}{16}\right) \mp \frac{1}{4}v$. But when v is small, we may take $y \mp v$ nearly $m \surd \frac{a}{2}$, and $z = \frac{ma \surd b}{m \surd (\frac{1}{2}a)} = \surd (2ab)$, and $x = a \pm \surd (2ab)$, while the height of a fluid, in which the velocity would be y, is nearly $a \pm \frac{3}{2} \surd (2ab)$: consequently, when the velocity v is at all considerable, y must be somewhat greater than the velocity of a wave moving on the surface of the elevated fluid; and probably the surface of the

elevated

elevated portion will not in this case by perfectly horizontal; but where v is small, y may be taken, without material error, $m \surd \frac{x}{2}$, or even $m \surd \frac{a}{2}$, which is the velocity of every small wave. The coefficient m is here assumed the same for the motion of a wave, as for the discharge through an aperture, and I have reason from observation to think this estimation sufficiently correct.

Case where the moving end remains open below.

Supposing now the movable end of the canal to remain open at the lower part as far as the height c, then the excess of pressure, occasioned by the elevation before it, and the depression behind, will cause the fluid, immediately below the movable plane, to flow backwards, with the velocity determined by the height, which is the difference between the levels; and the quantity thus flowing back, together with that which is contained in the movable elevation, must be equal to the whole quantity displaced. But the depression, behind the movable body, must vary according to the circumstances of the canal, whether it be supposed to end abruptly at the part from which the motion begins, or to be continued backwards without limit: in the first case, the elevation z will be to the depression as v to $y - v$, the length of the same portion of the fluid being varied inversely in that ratio; in the second case, the proportion will be as $y + v$ to $y - v$: and the difference of the levels will be $z + z \frac{y-v}{v} = \frac{zy}{v}$, or secondly $z + z \frac{y-v}{y+v} = \frac{2zy}{y+v}$; and first, $m \surd \frac{zy}{v} c + (y - v) z = (a - c) v$; but, since y is here considered as equal to $m \surd \frac{a}{1}$ putting $\surd \frac{a}{2} - \surd b = d$, $y - v = m d$, and, calling $a - c$, e, $m \surd \frac{zy}{v} c + m d z = m e \surd b$, $\surd \frac{zy}{v} c + dz = e \surd b$, $c^2 \frac{zy}{v} = e^2 b + d^2 z^2 - 2 dze \surd b$, $z^2 - \left(\frac{c^2 y}{a^2 v} + \frac{2 e \surd b}{d}\right) z = - \frac{e^2 b}{d^2}$, and, calling $\frac{c^2 y}{2 d^2 v} + \frac{e \surd b}{d}$, f, $z = f - \surd \left(f^2 - \frac{e^2 b}{d^2}\right)$: and in the same manner

f is

f is found, for the second case, equal to $\frac{c^2 y}{d^2 (y+v)} + \frac{e \surd b}{d}$. For example, suppose the height a 2 feet, $b = \frac{1}{4}$, $c = 1$, and consequently $e = 1$, then d becomes $\frac{1}{2}$, $v = 4$, and $y = 8$; and in the first case $z = {\cdot}1$, and in the second $z = {\cdot}14$.

Varieties of the velocity and open space.

If v, the velocity of the obstacle, were great in comparison with $m \surd \frac{a}{2}$, the velocity of a wave, and the space c below the obstacle were small, the anterior part of the elevation would advance with a velocity considerably greater than the natural velocity of the wave: but if the space below the obstacle bore a considerable proportion to the whole height, the elevation z would be very small, since a moderate pressure would cause the fluid to flow back, with a sufficient velocity, to exhaust the greatest part of the accumulation, which would otherwise take place. Hence the elevation must always be less than that which is determined by the equation $m \surd z c = e v$, and z is at most equal to $\left(\frac{e v}{m c}\right)^2 = \frac{e^2}{c^2} b$; but since the velocity of the anterior margin of the wave can never materially exceed $m \surd \frac{x}{2}$, especially when z is small, and $\surd \frac{x}{2}$ being in this case nearly $\surd \frac{a}{2} + \frac{e^2}{2 \surd (\frac{1}{2} a) c^2} b$, $m \surd \frac{x}{2} - m \surd b = m \left(\surd \frac{a}{2} + \frac{e^2 b}{\surd (2a) c^2} - \surd b \right)$ which, multiplied by z, shows the utmost quantity of the fluid that can be supposed to be carried before the obstacle. Supposing $b = \frac{1}{2} a$, this quantity becomes $m \surd \frac{a}{2} \cdot \frac{e^4}{c^4} \cdot \frac{a}{4}$; and if $\frac{e}{c}$ be, for example, $\frac{1}{10}$, it will be expressed by $\frac{1}{10000} a v$, while the whole quantity of the fluid left behind.

A contraction moving along an elastic pipe.

A similar mode of reasoning may be applied to other cases of the propagation of impulses, in particular to that of a contraction moving along an elastic pipe. In this case, an increase of the diameter does not increase the velocity of the transmission of an impulse; and when the velocity of the

the contraction approaches to the natural velocity of an impulse, the quantity of fluid protruded must, if possible, be still smaller than in an open canal; that is, it must be absolutely inconsiderable, unless the contraction be very great in comparison with the diameter of the pipe, even if its extent be such as to occasion a friction, which may materially impede the retrograde motion of the fluid. The application of this theory to the motion of the blood in the arteries is very obvious, and I shall enlarge more on the subject when I have the honour of laying before the Society the Croonian Lecture for the present year.

Motion of the blood.

The resistance, opposed to the motion of a floating body, might in some cases be calculated in a similar manner: but the principal part of this resistance appears to be usually derived from a cause which is here neglected; that is. the force required to produce the ascending, descending, or lateral motions of the particles which are turned aside to make way for the moving body; while in this calculation their direct and retrograde motions only are considered.

Resistance to a floating body.

The same mode of considering the motion of a vertical lamina may also be employed for determining the velocity of a wave of finite magnitude. Let the depth of the fluid be a, and suppose the section of the wave to be an isosceles triangle, of which the height is b, and half the breadth c: then the force urging any thin vertical lamina in a horizontal direction will be to its weight as b to c; and the space d, through which it moves horizontally, while half the wave passes it, will be such that $(c-d)\cdot(a+\frac{1}{2}b)=ac$, when ce $d=\frac{bc}{2a+b}$. But the final velocity in this space is the same as is due to a height equal to the space, reduced in the ratio of the force to the weight, that is, to the height $\frac{bb}{2a+b}$, and half this velocity is $\frac{1}{2}m\sqrt{\left(\frac{bb}{2a+b}\right)}$, which is the mean velocity of the lamina. In the mean time the wave describes the space $c+d$, and its velocity is greater than that of the lamina in the ratio of $\frac{c}{d}+1$ to 1, that is $\frac{2a+b}{b}+1$ or $\frac{2a}{b}+2$ to 1, becoming $m\left(\frac{a}{b}+1\right)$

Velocity of a wave.

b

$\frac{b}{\sqrt{(2a+b)}} = m \frac{a+b}{\sqrt{(2a+b)}}$; which, when b vanishes, becomes $m\sqrt{\frac{a}{2}}$, as in Lagrange's theorem, and, when b is small, $m\left(\sqrt{\frac{a}{2}} + \frac{3}{4}\frac{b}{\sqrt{(2a+b)}}\right)$, or $m\frac{a+\frac{3}{4}b}{\sqrt{(2a)}}$; but if a were small, it would approach to $m\sqrt{b}$, the velocity due to the whole height of the wave.

V.

A Mineralogical Description of the Mountain and Silver Mine of Chalanches, in the Department of the Isère. By HERICART DE THURY, *Mine Engineer*.*

Mountain of Chalanches.

THE mountain of Chalanches, noted in the annals of mineralogy for the variety and beauty of the mineral substances met with in it, is now become celebrated in metallurgy, for the abundance and richness of its silver ores.

Situation and height.

This mountain is in the vicinity of Allemont, in the canton of l'Oisans †. It is above the confluence of the Olle and the Romanche, 2 myriam. [about 12 miles] east of Grenoble in a straight line. Its height, taken at the buildings belonging to the silver mines, is 2159 met. [2359 yards] above the level of the sea; but some primitive peaks, stretching from south-east to north-west, rise 580 or 590 met. [635 or 645 yards] higher. Its loftiest summit is about 2750 met. [3005 yards].

Noticed by several, but imperfectly.

Many celebrated mineralogists have written on this mountain. Schreiber, director of the practical school of Mont-Blanc, who superintended the working of the silver mines at Chalanches several years with great success, has published

* Abridged from the Journal des Mines, vol. XX, p. 41.

† The canton of l'Oisans is the richest country in France with respect to its mineral substances. Its lofty mountains conceal a great number of veins, the various and superb products of which are daily enriching our finest collections.

many

many accurate and judicious observations on them in the Journal de Physique, but he has considered them merely as silver mines. De Bournon, in his mineralogy of Dauphiny, in the same Journal, has attempted to explain the origin of its veins, and given a very ingenious theory for this purpose; but unfortunately it is not applicable to the minerals of Chalanches. Dolomieu, Faujas de Saint-Fond, Dietrich, Mongez, Guettard, and others, have also spoken of this mountain, but no one has given a general view of its mineral substances.

De Bournon's theory of its veins inconsistent with facts.

Before I enumerate and describe the various products collected on the mountain of Chalanches, I shall give a geological sketch of it. It is of the primitive order, and composed of rocks, the base of some of which is simple, of others mixed. The latter are the most numerous, and constitute the chief mass of the mountain. The greater part of the veins yet known are found in a micaceous quartz rock, the strata of which dip in general to the south-west, but at an angle that frequently varies. The manner in which these rocks lie with respect to each other is pretty constant. Granite forms the base of the mountain, and is foliaceous. It frequently partakes of the nature of gneiss, sometimes of that of amphibolic rocks, and frequently of both at the same time. The gneiss and micaceous rocks vary as much in their grain and texture, as in the difference of their constituent elements. These rocks frequently alternate with amphibolic rocks; often they are mixed together; and still more commonly their association presents itself with all the characters of a granite, in which the mica abounds but little. In some places the gneiss contains iron pyrites, and occasionally calcareous particles, the presence of which is easily discovered by means of nitric acid. The colour of the gneiss varies extraordinarily. Gray, yellow, green, white, black, &c., are its most common tints: but it is frequently of a red, or reddish colour, whence the rocks it forms have been termed burnt.

Geological sketch of it.

Granite.

Gneiss.

Hornblende.

About a third of the way up the mountain, and 800 met. [874 yards] west of Traverse, the last hamlet we meet with before we get into the wood of the mine, we see in the road three strata of primitive limestone, which alternate with granitic,

Primitive limestone.

granitic, micaceous, and amphibolic rocks. These strata dip to the west at an angle of 60°. Their direction is north and south. This carbonate of lime is saccharoid, and emits a fetid smell when struck. Two of the three strata are a tolerably pure white; the third is gray with a rosy tinge, and adheres to the amphibole, that serves it as a wall. A little distance from this we find white feldspar rocks containing garnets; a little farther, gneiss with garnets; below, granitic rocks with tourmalines; and lastly, proceeding down to Allemont, the proper position of the secondary limestone on the primitive rocks,

Feldspar and gneiss with garnets. Granite with tourmalines. Secondary limestone.

Bent strata.

Toward the summit of the mountain we see quartzose and amphibolic rocks in strata that are bent, and turned back on themselves. Sometimes the doublings and redoublings are very numerous in the same mass.

Summit.

The summit of the mountain is schistose amphibole veined with quartz. It is naked, and in part destroyed, having fallen down in irregular blocks of different sizes.

Height.

The centre of the works is 1514 met. [1654 yards] above the confluence of the Romanche and Olle, or 2159 met. [2359 yards] above the level of the sea.

Ores.

The ores of Chalanches are disposed in veins, beds, and nodules; but these different modes are not continued on regularly.

Veins very irregular.

The veins are infinitely varied. Their magnitude, direction, and inclination vary continually, and are subject to numerous accidents. These veins are in general placed one above another; they are sometimes very near together; they cross each other in every direction; they preserve no regularity either in their course or dip; they frequently proceed in a direction opposite to what they took at first; lastly, they sometimes unite, and proceed together for a certain space, after which they separate, perhaps to unite again, or to disappear entirely, and with very different circumstances. The richness of the veins is not more constant than their mode of being. Frequently we find veins yielding 20 or 25 parts of silver in 100 of ore, which at a few decimetres [the dec. is near 4 inches Eng.] distance present nothing but a sterile gangue.

Beds the same.

The beds of ore are not so common as the veins; they do not

not continue for any considerable length; and they experience the same accidents. Their richness, direction, inclination, thickness, &c., are continually varying. They are incessantly intersected, turned aside, choaked, and interrupted by the veins. Finally, as they appear to owe their formation to the same cause as produced and formed the veins after the rupture and convulsions of the mountain, and the filling up of its clefts, I am induced to consider them rather as horizontal veins, than as real beds.

Sometimes the ore occurs in nodules, but less frequently than in veins, and like these they vary both in richness and magnitude. Nodules.

Their gangue is still more varied. Most commonly it is carbonate of lime, pure, mixed, associated, crystallized, amorphous, &c. Sometimes it is sulphate of lime; in other places it is flexible asbestus: frequently it is hyaline quartz, pure, mixed, crystallized, amorphous, &c.: occasionally it is argilaceo-calcareous: here it is green or brown pulverulent chrolite talc, and yonder the same in mass containing native silver: frequently it is epidote in mass or crystallized: &c. Among the metallic gangues we find oxide of cobalt both earthy and vitreous, arsenicated, arsenical, and gray cobalt, all of them more or less argentiferous. Arsenical and carbonated nickel frequently perform the office of a gangue, and sometimes the first of these is even rich in silver. Arsenic too is found in the argentiferous ores, but it is more rare than the preceding. Antimony occurs native, sulphuretted, oxided, and as a hidrosulphuret. Copper, which is very abundant in the veins, is met with in different states: pyritous, sulphuretted, oxided, and hidrosulphuretted. Gray copper ore is frequently found in the asbestus, and always very rich in silver. Iron and manganese, both in the state of oxide, occur very commonly in the veins at Chalanches: and the latter is even one of the richest gangues in silver. Lead is found as a sulphuret, and sometimes phosphated. Mercury too is found in the gangues at Chalanches, and frequently is even abundant in them.

Gangue very various.

Metallic gangues.

Above the silver mines we find two narrow valleys, one of which runs east, the other west. These narrow gradually to the foot of the highest peaks. Valleys.

The

Anthracite. The western valley, called the Clos du Chevalier, exhibits an interesting object of study, a stratum of anthracite between strata of clay schist with vegetable impressions deposited on a granitoid breccia. The latter immediately covers the primitive rocks of gneiss or amphibole, that contain the veins of silver.

High ridge. Lastly, on the north of Chalanches is a sharp ridge, that joins the loftiest peaks of the great chain of Belledonne, which extends from Vizille to Allevard as you ascend toward the high mountains of Maurienne. This chain is distinguished for the number and variety of its rich metallic veins.

Chalanches an interesting object of study. The geology of the mountain of Chalanches offers us grand facts, and various subjects of observation with respect to the catastrophes, that have destroyed and overturned its primitive organization; the violence it has subsequently undergone; the number of its veins, their formation, and continual changes; and the assemblage of so many various substances, separate or combined together, and modified in numberless ways. Of these substances I shall proceed to give a brief methodical sketch, arranged according to the four grand divisions of the system of Haüy.

Sketch of its minerals.

Haüy's 1st class. Carbonate of lime. In the first class, acidiferous earthy substances, we have carbonate and sulphate of lime. The carbonate is frequently found crystallized, and exhibits numerous varieties of form and colour. It is still more various in its combinations. Some of its compounds with iron and manganese have been wrought as sparry iron ore. It is mixed with magnesia in large irregular crystals disseminated through steatitic, asbestous, chloritic, and magnesian gangues.

Sulphate of lime. The sulphate has often been found in transparent crystals, sometimes coloured by copper, and lying on a silky amianthus, partly white, and partly rosecoloured from a mixture of pulverulent arseniate of cobalt. These are uncommon, and varied in a pleasing manner.

Sulphate of barytes. At the foot of the mountain, toward Allemont, is a vein of sulphate of barytes, which was formerly explored in search of sulphuret of lead, that appeared there to the day. It has some varieties of form and colour, being radiated, granular, compact, and white.

Of

Of the second class, earthy substances not acidiferous, quartz is tolerably abundant. It has been remarked, that its presence is a bad omen with regard to the richness of the silver ore. Its most usual combinations are with cobalt and antimony. The hyaline quartz is pretty frequent in the veins, but rarely in well defined crystals: it is combined with a great variety of earthy substances, but its combination with metals are still more numerous. Its compounds contain from two to five substances, and even more. 2d class. Quartz. Rock crystal.

Jasper is not common, but is sometimes met with in the metallic veins, or those of quartz. Its varieties are brown, reddish, yellowish, and blackish. Jasper.

Garnets are very common in the rocks of white micaceous feldspar, but they are very small, being at most 2 millim. [0·78 of a line] in diameter. Those that are found in the gneiss are larger, but less distinctly formed. They have not many varieties either of figure or colour. Garnets.

The veins produce some handsome varieties of feldspar, which is tolerably abundant in some parts of the mountain, and frequently in well defined crystals. Feldspar.

Tourmalines are found in a rock of white micaceous feldspar, in tracing a vein of sulphuret of lead at Lafare. They are in very distinct crystals several centimetres long, and about 1 cent. [3·9 lines] in diameter. They frequently occur of a cylindroid figure in the heart of the solid rock. Tourmalines.

A few small veins of axinite associated with epidote are found in a hornblende rock at the foot of the mountain near the cascade of Batou. Violet schoerl.

Epidote is very abundant in the veins, sometimes well crystallized, and of a fine deep green, but very brittle. More frequently it is in mass, and sometimes constitutes whole rocks. Most commonly however it lines the sides of clefts in hornblende quartz rocks. Green schoerl, or pistacite.

Amphibole and actinote, which the analyses of Mr. Laugier have united into one species, are abundant, and form masses of considerable bulk. At the bottom of the mountain, under the vein of Lafare, where a considerable portion of cliff fell down some years ago, there is a rock of white or gray feldspar, containing some beautiful needles of amphibole of a blackish green. This rock exhibits one Hornblende & strahlstein.

 of

of the most beautiful varieties of syenite granitello. Two associations peculiar to the amphibole of Chalanches are, 1, a calcareo-siliceous titanite in crystals of a lemon yellow, which form a pleasing contrast with the dark green of the amphibole in laminar masses: 2, a magnetic oxidule of iron in indeterminate crystals of a black metallic brown with a laminar fracture.

An ore of titanium, and of magnetic iron.

Some veins of arseniated cobalt ore very rich in silver, and even frequently presenting this metal in its native state, contain a mixture of small greenish crystals, which might be taken at first sight for granulous epidote, but appear to me to be peridot. I could not observe any distinct figure in them: they are disseminated in small irregular grains amid arseniated or oxided cobalt; frequently they are with sulphuretted silver; and sometimes they are covered with native silver. This mixture is one of the least common at Chalanches, but it is one of the most remarkable for the variety of its constituent substances, and its richness in silver amounts to 18 or 20 hect. of metal in a myriagr. of ore [18 or 20 per cent].

Chrysolites.

Mica is very abundant in the rocks of Chalanches. It is in hexaedral laminæ frequently very well defined; and in colour white, yellow, gray, or blackish. The celebrity of the mines of Chalanches, and the appearance of the great quantity of silvery white mica in its cliffs and declivities, have led many persons but little versed in mineralogy to suppose, that the whole mountain was formed of silver.

Mica.

Asbestus is very common in the veins at Chalanches. It is found in different states, and modified by various mixtures. Frequently it is very rich in silver, and thrown into the furnace with the ore.

Asbestus.

The amianthoid of Chalanches is in fine, slender, silky needles, sometimes stiff and elastic, of a silky green colour. It is frequently tinged by oxide of iron, or of manganese.

Amianthoid.

Chlorite talc is very abundant in the veins, both in compact masses and pulverulent; and in each state it frequently contains native silver.

Chlorite.

In the third class, combustible substances not metallic, there are only sulphur and anthracite. Sulphur is very common

3d class. Sulphur.

common as a mineralizer, and has sometimes occurred native in a whitish yellow powder. The anthracite is found foliated, scoriform, striated, and earthy; and is frequently traversed by small veins of quartz, exhibiting very pretty crystallizations, that form a striking contrast with it. Anthracite.

We now come to the fourth class, that of metallic substances. 4th class.

Gold has been reported to have been found at Chalanches native, or alloyed with native silver, but this is erroneous. Mr. Schreiber however, in his analysis of the copper pyrites of the vein of St. Lewis found it to be auriferous. Gold.

Native silver is very abundant in the veins at Chalanches. It has never been found crystallized, but in various forms, as branchy, filamentous, capillary, lamellar, granulous, amorphous, pulverulent, and earthy. These eight varieties are mixed with a number of different substances, and in particular with carbonate of lime, quartz, epidote, chlorite, peridot, copper, arsenical nickel, carbonated nickel, earthy oxide of cobalt, arseniated cobalt, oxide of iron, oxide of manganese, oxide of mercury, lead, antimony, &c. The native silver is found sometimes mixed with only one of these substances, at others with two or three of them, but more frequently with all of them together. Native silver.

Antimonial silver occurs very rarely. Antimonial silver.

Sulphuretted silver is very rare in well defined crystals: but it is found lamellar, filamentous, amorphous, and scoriform, disseminated in the same gangues as the native silver, and with the same mixtures. Sulphuretted silver.

Muriate of silver has never been found crystallized but once, when it was in a perfect cube, and the first time it had been noticed. It has since occurred frequently in a violet powder on the surface of native silver, or argentiferous cobalt ores; but it never forms more than a very thin coat on it. Horn silver.

Sulphuret of silver and antimony is found occasionally in small amorphous masses. Carbonate of lime tinged with oxides of iron and manganese are its most common gangues. Sulphuret of silver and antimony.

Beside these five states silver has frequently been found in sulphuret of lead, gray copper ore, arsenical nickel, &c.; Argentiferous ores.

 but

but it is seldom if ever visible in these ores, and only to be detected by an assay.

Mercury. Mercury has been found here native but once. It was in a calcareous gangue, coloured by sulphuret of mercury and oxide of manganese.

Cinnabar. Sulphuret of mercury is very common among the argentiferous ores. Frequently it is so concealed by the oxides of iron, manganese, and cobalt, that it is only to be found by assaying. It occurs likewise with the sulphurets of zinc and lead, and some others.

Galena. Sulphuret of lead is not abundant, but, when it does occur, it is moft commonly very rich in silver. It is frequently iridescent.

Carbonate of lead. Carbonate of lead is very rare. It is found in the cavities of sulphuret of lead, quartz, and some argentiferous ores. It is seldom crystallized; and when it is, it has the trihexaedral form. Most commonly it is acicular, and sometimes earthy.

Arseniated lead. Arseniated lead is found pulverulent or earthy in cavities of sulpheuret of lead. It is but rare.

Phosphated lead. Phosphated lead sometimes occurs in small, fine, distinct needles of a yellowish green colour, on the surface of sulphuretted lead that is full of hollows.

Molybdate of lead. Molybdate of lead was found by Schreiber at the foot of the mountain, near the cascade of Batou, after a great landslip. It is in a fine greenish schistous hornblende rock mixed with feldspar, intersected by small veins of green epidote, frequently crystallized in fine transparent needles. Among these needles the molybdate of lead is found.

Gray copper ore. Gray copper ore is very abundant, but never crystallized. It is commonly very rich in silver; and among the combinations for which it appears to have a preference is that of silky asbestus.

Copper pyrites. Pyritous copper sometimes constitutes veins of itself. A few of these were auriferous.

Carbonate of copper. Green carbonate of copper is frequent in the argentiferous mixtures. It is commonly superficial, or as a colouring principle.

Nickel. Some of the veins have yielded fine and rich specimens of nickel,

nickel, in which silver has frequently formed more than one sixth of the mass.

Arsenical nickel occurs very pure in nodules, though rarely. It is more frequently mixed with cobalt, iron, and silver. Frequently it contains silver enough to be worth working as an ore. This is a combination peculiar to Chalanches. It is in irregular nodules, of different sizes, capable of being cut and polished. Arsenical nickel.

Oxide of nickel occurs frequently in fine specimens commonly covering arsenical nickel. It is white, or of a green more or less deep, and always pulverulent. Oxide of nickel.

Magnetic oxidulated iron is frequent in the hornblende rocks. It is in crystals of an indeterminate form, of a deep black colour, and with somewhat of a metallic lustre. Magnetic oxide of iron.

Oligist iron is very abundant in some veins, is frequently found in the strata of gneiss, and is common in those of quartz, and carbonate of lime. Specular iron ore.

Sulphuret of iron is not abundant, but sometimes occurs in the veins, and in the rocks. Occasionally it is without its yellow colour and metallic lustre, having acquired a dull and earthy aspect from decomposition. Pyrites.

Oxided iron is the most common gangue of the silver ore. Sometimes the silver can be discerned in it by the naked eye; but it is more commonly concealed by the iron, which is more or less coloured and mixed with foreign matters. Oxide of iron.

The sparry iron ore has been mentioned already under the head of carbonate of lime. Iron spar.

Manganese is very abundant in the veins, but hitherto it has been found only in the state of oxide. It accompanies the richest silver ores. Manganese.

Oxide of zinc occurs, though rarely. Sulphuretted zinc is rather more common. Neither of them is crystallized. Zinc.

Arsenical cobalt is scarce, never forms veins alone, and commonly is destitute of silver. Arsenical cobalt.

Gray cobalt is rather more common, but seldom pure. It is frequently mixed with arsenical nickel, and sometimes with silver. Gray cobalt.

Black oxided cobalt is very abundant, sometimes pure, but commonly mixed with oxides of iron and manganese, sulphuret of mercury, and often silver. Black oxided cobalt.

Arseniated

Arseniated cobalt.

Arseniated cobalt is met with in some veins, but never forms veins of itself. It pretty frequently indicates the proximity of veins of argentiferous oxide of iron. The argentiferous earthy arseniate of cobalt at Allemont contains from 1, 2, or 3 ten thousandth parts of silver to ten or twelve per cent.

Native antimony.

Native antimony is very rare at Chalanches. It is in large or small shining metallic scales confusedly arranged, and forming masses capable of being divided parallel to the faces both of a regular octaedron and a rhomboidal dodecaedron. Sometimes it is in solid compact nodules capable of being cut and polished. The presence of arsenic, though in very small quantity, changes entirely the texture of the antimony. In this state it forms a kind of scales, the surface of which is frequently undulated.

Sulphuret of antimony.

Sulphuretted antimony is found in the same situations as the preceding, but it is infinitely more scarce. It is prismatic or laminar, but more commonly amorphous.

Oxide of antimony.

Oxided antimony occurs in rectangular crystalline laminæ in the interior of the native antimony; but more frequently pulverulent, white, and earthy, on the surface of the nodules with large facets.

Hidrosulphuret of antimony.

Hidrosulphuretted antimony is still more rare than the preceding species, but it is found with the oxide in the superficial cavities of the native antimony with large facets.

Arsenic.

Arsenic is never found native, but is sometimes combined with the nickel, cobalt, or antimony.

Titanium.

The greenish or blackish amphibolic rocks frequently contain lemon-coloured crystals, which have been found to be a siliceo-calcareous ore of titanium.

VI.

Effects of Gravity on the Balance of a Watch compared with those on the Pendulum of a Clock. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Effect of gravity on the ba-

(*a*) I Have been often led to compare the balance of the watch with the pendulum of the clock. The functions of both

both being to regulate the motion of the respective instruments, similar methods have been adopted in each for compensating the effects of heat and cold. But the difference of latitude which has been so fully recognized as affecting the pendulum has not yet, as far as I know, been considered to have any influence on the balance.

lance hitherto neglected.

(*b*) The time of the fall of the pendulum is in the inverse subduplicate ratio of the force of gravity. In the watch, on the other hand, the time of the vibration of the pendulum spring is in the direct subduplicate ratio of the weight of the balance. It appears to me, therefore, that any approach toward the equator, by diminishing the gravity of the balance, must tend to accelerate the motion of the watch in the same ratio by which it retards that of the clock.

Gravity should accelerate the motion of a watch.

(*c*) Suppose the diminution of gravity at the equator $\frac{1}{229}$ $= 0{\cdot}004367$. Let the weight of a balance vibrating mean time at the pole be denominated 1. Being removed to the equator, its weight will be $1 - 0{\cdot}004367 = 0{\cdot}995633$, and the time of one vibration at the pole, to the time of one vibration at the equator, will be as $\sqrt{1}$ to $\sqrt{0{\cdot}995633}$, or as 1 to $0{\cdot}997814$; consequently the number of vibrations in one day at the equator will be to those at the pole as $\frac{1}{0{\cdot}997814} = 1{\cdot}002191$; which reduced to time gives $24^h\ 3'\ 9{\cdot}3''$. That is, the watch will go too fast $3'\ 9{\cdot}3''$. If the diminution be taken at $\frac{1}{190}$, agreeing with a printed table of the variation of the pendulum to every fifth degree of latitude, then by the same process, the errour of the watch will be $3'\ 48{\cdot}3''$ daily.

Difference of rate at the pole & the equator.

(*d*) The errour here is so great, that in proceeding from this latitude to the line, it is not possible, that it could have escaped observation, with the almost numberless experiments that are now made with watches; but if the reasons for the variation of the balance be found just, these experiments will stand in opposition to those that have been made with the pendulum. With respect to the latter, they have been conducted by men of such eminence, that we should have no reason to doubt their accuracy, if they did not differ so widely from one another.

Experiments with the pendulum should differ from those with the balance.

(*e*) Mr. Richer found by experiments, that a pendulum, which

Experiments with the pendulum do not agree.

which vibrated seconds at Paris, required to be shortened a line and a quarter at Cayenne, four degrees from the equator. Mr. Couplet the younger observed, that a pendulum, which swings seconds at Paris, must be shortened $2\frac{1}{2}$ lines somewhere in Portugal, which is more than Sir Isaac Newton allowed from the pole to the equator. Messrs Picart and de la Hire found the length of the pendulum, which beats seconds, the same at Bayonne, at Paris, and at Uranibourg in Denmark. Mr. Cassini pretended to prove from experiments, that the polar diameter is the longest. In short, conclusions can hardly differ more widely.

(*f*) It may be thought, that a decrease of gravity will not have the same effect as lessening the weight of a balance. I suppose gravity constitutes weight. They decrease and vanish together. It is the inertia of the balance, that regulates the vibrations of the spring; but its inertia arises from its weight. They decrease and vanish together. I do not mean however to assert, that there is no variation of gravity in different latitudes. I only suspeet, that it is considerably less than the received opinion allots to it, and that it is probably irregular. The centrifugal force about the equator is a principle evidently tending to lessen the effect of gravity; but the internal structure of the Earth, of which we know nothing, may lessen or vary its effect.

Experiments with the pendulum and balance may be made within 150 miles.

(*g*) Those who have inclination and opportunity to make experiments or observations on this subject will have no occasion in the first instance to go any great distance. Two degrees about this latitude, according to the table, produce not less than $7\frac{1}{2}''$ per day of errour in the pendulum; and if the balance be found as accurate a measure of the variation of gravity as the pendulum by operating the contrary way, there will arise a difference between them of $15''$ per day; a quantity very discernible. Many watches are to be found, that do not vary in their daily rate so much as $7\frac{1}{2}''$ for a considerable time, and clocks not nearly so much.

Portable pendulum rod in different pieces.

(*h*) A pendulum rod might be made more portable, by being in three pieces to join together with perfect accuracy, and consequently not subject to be bent, by which the length would be altered; and the ball being also fixed fast, and the time of the vibration ascertained in some latitude, it

it might be taken in pieces, and when reconstructed elsewhere the difference of time would show the length requisite more correctly than any other sort of measurement, and consequently the difference of gravity. Several little errours would be avoided by always using the same pendulum.

(*i*) Mr. Cumming, in his Elements of Clock and Watch Work (notes to Article 410) says, "In the pendulum, gravity is the motive force; and in the balance the spring: the vis insita is the resistance in each; and the contrary: therefore, when the motive force is in each as the resistance, the velocities and times must be equal. Hence it also happens, that (*c. p.*) the balance measures the same time in all latitudes." And afterward: "Here by altering the weight, is strictly meant, altering the vis inertiæ; for the vibrations of a balance, whose centre of gravity coincides with its centre of motion, have not the least dependence on gravitation, otherwise it would alter its times in different latitudes as well as the pendulum."

Vibrations of a balance do not depend on gravity.

(*k*) Here it appears to me, that Mr. Cumming considers the vis insita and vis inertiæ of matter, as powers wholly independent of its gravitation or weight. I suppose it will not be doubted, that a balance becomes heavier or lighter, as gravity is greater or less, and if hung by its spring, it will stretch the spring proportionally to the power of gravity: still he considers its vis insita to remain unaltered with its motive force, and consequently that it will measure the same time in all latitudes.

(*l*) I think it is generally allowed, that the vis inertiæ is proportional to the quantity of matter. We distinguish a greater or less quantity of matter by its weight, consequently its vis inertiæ will be proportional to its weight, for it is certain that any thing heavy has more resistance than a thing that is lighter.

Vis inertiæ.

(*m*) Suppose a watch vibrating with a steel balance, and, without any other alteration, let it be taken off and replaced with a gold balance of equal magnitude in every way, which, for convenience we shall suppose to be just double in weight. Though in this case nothing is altered but the weight, the inertia is just doubled, for it will require another spring equally strong, added to the former, to give it the same velocity.

Balances of different weights require springs of different strengths.

(*n*) Again

Enlarging a balance has a similar effect.

(*n*) Again, every thing remaining as before; suppose I beat out this gold balance till its diameter be just doubled, or, more accurately, till its diameter of percussion is just double: here the inertia is quadrupled, for now it will require eight springs equally strong with the first to make it vibrate in the same time, and generally, let D be the diameter of percussion, and W the weight, the power of any balance will always be as $D^2 \times W$, for the strength of the spring must be in this proportion to make it vibrate in the same time. If the number of vibrations are changed, that is more or less in the minute, the velocity in similar parts of each vibration will be as this number, and if it be denoted by N, the power of any balance will be as $\overline{N^2 \times D^2 \times W}$, the spring being in this proportion.

(*o*) Mr. Cumming goes on to say, that "the vibrations of a "balance, whose centre of gravity coincides with its centre "of motion, have not the least dependence on gravitation."

Surely this cannot mean, that balances of different weights, having their spring or motive force the same, will vibrate in equal time; but either this must be the case, or he must consider a variation in the power of gravity as not altering the weight of any thing.

Variations of timekeepers at sea.

(*p*) I have heard it remarked, that timekeepers do not go so well at sea as they are found to do on land. But as all considerable changes of place are made by sea, it is possible, that the influence of latitude on gravitation may be the unsuspected cause of some part at least of this deviation. If the principle I have suggested be not erroneous, it will be of more importance than ever, to ascertain the exact variation of this influence. But as I am very conscious, that my attainments in science are not such as to entitle me to be positive in a matter of this magnitude, however plain it may appear to my own mind, I am solicitous to be favoured with your judgment upon it, or that of any of your correspondents.

I am, Sir,

Your most obedient humble servant,

X.

ANNO-

ANNOTATION.

We assert, that the gravity of a body, is as its mass, and that its mass is as its inertia, or the power by which it tends to preserve its state as to motion. Admitting or assuming the inertia to be invariable, we deduce, that gravitation varies inversely as the square of the distance between gravitating bodies; and our observations on the relative situations and motions of the heavenly bodies show, that this is the case; or else, that, if the inertia be subject to variation, its changes are such as to produce, along with some correspondent opposite change in gravity, a result of the same kind, as was accounted for on the supposition that the inertia does not vary. But we seem to have no decisive facts to determine as to our original hypotheses, and therefore prefer the simplest. Though the author of the preceding letter appears to have confounded the terms gravity and inertia; yet the experimental speculation, to which his letter points, appears to be of sufficient interest to lead to meditations of some value, and to authorize its insertion in our Journal.

Inertia considered as invariable.

But decisive facts are wanting,

so that there is room for inquiry.

W. N.

VII.

An Account of a simple and economical Method of preparing an artificial Cheltenham Water highly impregnated with Carbonic Acid (fixed Air). By RICHARD GREENE, *Esq. of Cork, A. B. Trin. Col. Dub., M. D., and President of the Royal Medical Society Edinburgh.*

To Mr. NICHOLSON.

SIR,

IF you think the following communication merits a place in your valuable Journal, it is perfectly at your service, and by its insertion you will confer an obligation on

Cheltenham water of high repute.

Your most obedient humble servant,

Edinburgh, Dec. 27, 1808. R. G.

The purgative and chalybeate waters of Cheltenham have long and deservedly been celebrated, in the cure of many obstinate and alarming diseases. The chief obstacles to their more general employment seem to be, the impossibility

lity of procuring the natural water in a proper state except at the spring; or the scarcity and exorbitant price of the aërated artificial imitation, which too is very seldom to be met with properly prepared.

The carbonic acid of essential importance in it.

The simple solution of the saline matter of these waters is often prescribed with the most happy effects. This, however, entirely wants one very active and important ingredient of the natural water, the carbonic acid; by the lively pungency of which, when in sufficient quantity, the offensive taste of the salts is concealed, and a nauseous medicine is converted into an agreeable beverage. This is only one, and perhaps the least of the advantages of the carbonic acid. By virtue of it, and the iron which it holds in solution, the use of this water may be persevered in for a long time without inconvenience: for during its employment the appetite will be improved, and the digestive organs strengthened:

The more of this it contains the better.

and the greater quantity of this acid the water can be made to contain, the less offensive will it be to the palate, and the less it will be liable to be rejected by the stomach, while the whole system, sympathizing with the tonic effects it produces on that organ, will be the more speedily invigorated.

Its chief uses.

It would be out of place here, to enter largely into the medicinal properties of this water*, I shall just enumerate the principal affections of the system, in which it has been found particularly useful. Those, whose biliary organs a long residence in a warm climate has impaired, seldom fail to receive much benefit from a course of Cheltenham water, and its use may be continued even under circumstances of great debility: and will be eminently serviceable if any symptoms of dropsy or anasarca threaten, as so often happens in affections of the liver. In glandular obstructions its use has often been attended with success. In all cases where the secretion of bile is vitiated, or irregular; and in jaundice from resistance to a free discharge from the gall bladder, attended with sense of heat and distention after eating, this water will be employed with particular advantage. For removing a sense of fulness about the head in the plethoric, and for carrying off the effects of any excess in eating or

* See Dr. Saunders's Treatise on Mineral Waters, 1805.

drinking

drinking, no medicine seems more proper than this laxative diluent. For habitual constipation of the bowels, as the stimulus of this water is so slight that its frequent repetition cannot be productive of injury, it may be considered the safest as certainly it is the most pleasant remedy.

An artificial water, equal if not superior to the natural, easily prepared.

From the many valuable properties of this mineral water, I am firmly persuaded, that it would be far more extensively employed than it is at present, if it were possible to convey the natural water to a distance unimpaired, or to procure at a reasonable price an imitation of it, that could be relied upon. I therefore trust, it may not be altogether useless, to lay before the public a simple and economical method, by which any person may in a few minutes prepare a water eminently possessing the virtues of the natural spring, as composed of the same ingredients, but more palatable, as containing a larger quantity of the carbonic acid, which also increases its tonic powers.

Ingredients of the natural water.

The natural Cheltenham water owes its virtues principally to the sulphates of soda and magnesia *(vitriolated natron, and vitriolated magnesia)* which are its purgative constituents. It contains also about one eighth of its bulk of carbonic acid, which holds in solution nearly five grains of iron in each gallon of the water. The sulphate and carbonate of lime, which may also be detected in it, are not likely in any respect to influence its medicinal properties.

Impregnation of water with carbonic acid.

The carbonic acid employed in preparing artificial mineral waters is obtained by decomposing the natural carbonates of lime *(marble or chalk)*, by means of the sulphuric *(vitriolic)* acid, which disengages the carbonic, in its elastic form, to be afterward forced into the water by mechanical pressure, and apparatus which has not yet been disclosed to the public.

Carbonates of the bases in Cheltenham water give out this acid when employed to compose the neutral salts in it.

The carbonates of soda and magnesia (common soda or prepared natron, and magnesia alba) contain a large proportion of condensed or combined carbonic acid essential to their constitution; the former about one sixth, and the latter nearly half its weight. They likewise are both decomposed by a proper quantity of the sulphuric acid, which, uniting to their bases the soda and magnesia, forms the sulphates of these bases, or the salts of Cheltenham water; the carbonic acid escaping with effervescence in its aëriform state.

and this may be retained in the water

It occurred to me, that if by any means this gas could be confined at its first disengagement, and a sufficient quantity of water present, it would be absorbed by that very water in which it would be disengaged, and serve the same purpose as when afterward driven into it by the force of a condensing apparatus. I made many experiments to this effect, with different proportions, and with different quantities of the ingredients, and with the most satisfactory results: and found, that the entire process could be safely and conveniently conducted, in a pint, or half pint bottle of common green glass. Having calculated what quantity of the carbonates of soda and magnesia *(properly proportioned)* would yield on decomposition a volume of carbonic acid, that might without danger be confined in a half pint bottle; I found the quantity of dilute sulphuric acid necessary to decompose it, and measured it out in a graduated glass. I then put the carbonates into the bottle, together with one grain of the sulphate of iron *(green vitriol)*, and nearly filled it with cold water; just leaving room to add the sulphuric acid, which I next put in, and instantly closed the bottle, securing the cork with a string. By a little agitation, the salts were soon dissolved, and the liquor became transparent: and on removing the cork, I had the satisfaction to find the process had succeeded equal to my most sanguine expectations: for the liquor was so highly impregnated with the gas, as to be scarcely distinguishable in taste from the best prepared soda water, and it flowed with ebullition from the bottle, the instant the string was untied, driving out the cork with considerable violence. The quantities here employed were too great for general use, being nearly one half greater than the proportions, which farther experience has enabled me to recommend: however, they show, that the strength of the water may be much increased without bursting the bottles.

In a common pint bottle. Experiment proving this.

Farther particulars of the process, and cautions to be observed in conducting it.

Process for making Cheltenham water.

The carbonate of soda employed was the common soda of commerce, that prepared from the muriate of soda *(sea salt)* by the patent process, which neither deliquesced nor effloresced by exposure to the air; and by a very delicate test

test gave no indication of the presence of lead, which has been suspected to be contained in soda manufactured in this way. The quantity I would recommend is two scruples, or forty grains troy, for the half pint of water. The carbonate of magnesia I made use of I prepared myself from sea water, and therefore I could depend upon its purity. The quantity for the same dose of water is one scruple, or twenty grains: the half of the quantity of the soda. The sulphuric acid I employ is the dilute acid of the Edinburgh College; of the specific gravity of 1·0735, and was formed, by adding one part (by weight) of the strong acid of commerce, of the specific gravity of 1·852, to eight parts of water. The quantity of this acid I use is 250 grains by measure, or a quantity equal in bulk to 250 grains of water. In this proportion, there is a slight deficiency of sulphuric acid, which should always be observed; for the excess of carbonic acid will be able to dissolve any carbonate of magnesia that may remain, and any excess of sulphuric acid gives the water a harsh disagreeable taste, and might injure the teeth, or affect the bowels, if irritable. The volume of carbonic acid gas disengaged by these quantities is about 35 cubic inches, and in a half pint bottle is equal to the active pressure of an additional atmosphere and a half on the sides of the bottle, or to two atmospheres and a half upon the surface of the water. The sulphuric acid is best to be diluted, as by this any errour in measuring will be diminished, and by dilution it throws down a sediment, which often contains a portion of lead.

Indication of deficiency of soda or of acid.

As the carbonate of soda is unsteady in the quantity of water it contains, being affected by the dryness or humidity of the atmosphere, and the sulphuric acid of commerce is not always of the same strength, it may happen, that the quantity of the acid I have here specified shall not exactly answer: if so, any excess will manifest itself by an acid taste in the water after it has stood some time exposed to the air in an open vessel; and any great deficiency of acid, by the whole of the magnesia not being dissolved. In this case the quantity of acid must be increased or diminished as indicated. But I would advise every person, who would wish to give this process a fair trial, to appreciate by experiment

The proper proportions best ascertain-

ed by an easy experiment.

riment the quantity of acid required for the soda and magnesia he may employ; as it is very simple, and easily performed. Weigh out the proper quantity of the carbonates, and put them into a common wine or ale glass, with about an ounce of water; add gradually the acid to them, constantly stirring them until they are dissolved, and all effervescence has ceased; taking care, that no excess be employed, which may be known by the acid taste, or by turning to a red the natural blue vegetable colours, (as infusion of litmus, or of red cabbage leaves in hot water); note exactly the quantity of acid that was used, and make a measure containing ten drops less than that quantity, which is easily done by marking a slender ounce phial or glass tube, by means of a file, or a diamond. If this is found to answer well, a good parcel of the same ingredients as those of the experiment should be procured and kept in close vessels.

Farther precautions.

The corks should be previously fitted to the bottles in which the artificial water is to be prepared, that no time may be lost after putting in the acid; and the bottles should always be kept on their sides, that the liquor may cover the corks, and thus prevent the escape of the gas. The magnesia should be in fine powder, that it may dissolve more readily: the soda is best not to be so, as by its slow solution, the magnesia will meet the acid in a more active state, and there is no danger, that the soda will not dissolve readily enough. By minding these precautions, a bottle of the water may be made ready for use in less than a minute: but it will keep very well, only observing to shake the bottle before opening it, as the iron precipitates in part.

Strength of the water may be varied.

The strength of the water may be varied at pleasure, by altering the quantities of the salts with respect to the water, but minding the relative proportion of the acid to the carbonates. For a pint bottle the quantities are: carbonate of soda four scruples, or eighty grains; carbonate of magnesia two scruples, or forty grains; sulphuret of iron two grains. This is a full dose for an adult, and more than should be taken for a constancy, during a course of this water. The expense of preparing a pint bottle is about one penny; the strong sulphuric acid or oil of vitroil, is only eight pence per pound, and the soda the same price, or nine pence, by the single pound.

General proportions and price.

VIII.

VIII.

Second Letter on the Advantages of Coal Gas Lights. By Mr. B. Cook.

To Mr. NICHOLSON.

SIR,

On the advantages of gas light.

IN my last letter I promised you a few plain drawings, but I have been out of Town nearly three months since I wrote to you, which has hindered me from sending them. I am desirous, however, if possible, to hand you the few following remarks, in addition to my last letter, in time for your Journal.

Where no soldering is required.

First, with respect to oil and cotton, the man who does not use these articles, may unthinkingly strike that out of the statement, and then there appears but little or no advantage from the adoption of the use of the gas lights. But he must not do this: for, if he do not use these articles, a large portion of the expense in the execution of the apparatus is saved, and one lamp will consume as much gas as nearly twelve lights, or candles. If this is duly considered, the saving is the same, or nearly so; for less gas will be wanting to be made, less coal consumed, and the man or boy, that makes the gas, will perhaps be required to make it only two or three times a week, instead of every day, which will be the case, if the gasometer is small and lamps used. Instead therefore of employing a man a part of his time at five shillings per week, a boy will be employed to do it at perhaps two shillings per week or less. Indeed in most manufactories a boy or an old man is kept to job, or go on errands; and he will be able to do this and all his other jobs besides; so that in most manufactories this man, which forms a very great portion of the expense, may be entirely struck out. Besides, the industrious man, who works in his own shop, may do it himself, as the trouble is little, only putting a fire under the retort, and filling the retort with coal; when it will require no farther attention, except to see now and then that the fire is kept up.

Leaden pipes preferable to old gun barrels.

Secondly, I recommend the use of old gun barrels for pipes: but since I wrote my former letter, I have been induced in a great measure to change my mind; for, having been employed in putting up the apparatus for some persons in this place, I find the promised advantage is not so great as it first appears. Being in short lengths, they require many joints, or soldering places, and if they be not properly sound and air-tight, the gas escapes, a disagreeable smell is produced, your gas is wasted, and the process is disliked. Now the patent lead pipe may be had in lengths of fifteen or twenty feet, so that even in extensive manufactories but few joints will be wanting. The pipes likewise can be made three quarters of an inch or one inch in the bore or hole, which is an advantage; you can bend them round any angles; and there will not be so great a probability of leakage, when there are but few joints. I think therefore, although the lead pipes will be rather more expensive, yet when all their advantages are considered, it will be better to adopt them, than the other. Besides, when the pipes are done with, and a man gives over trade, or when he removes his manufactory, he can very easily remove these and put them up again, or sell them for a great part of the original cost for old lead. There is also another saving in the workman's putting up the apparatus, as he puts up the lead pipes in half the time he would the old gun barrels.

Perhaps therefore we may make the following statement, for in fact it will be in general the true statement, where only lights are wanting for twenty weeks in the year.

Corrected statement.

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Coal for twenty weeks, at 1*s.* 3*d*	1	5	0	Twenty weeks, at 1*s.* 8*d.*	18	0	0
Interest on £20 or 25............	1	5	0	Coak...........	0	12	6
Saved...........	16	2	6		£18	12	6
	£18	12	6				

Here I have only allowed the half of my former statement for the expense of coal, supposing no soldering is done, for in that

that case not half will be used, and twice or three times a week will be quite sufficient for making the gas.

I have also left out entirely the expenses of a man, as this will be seen to be quite unnecessary, so little time will be required to make and attend it, if only used for lights.

The apparatus too in this case will not cost more than twenty or twenty-five pounds, so that I have allowed only one pound five shillings for interest. And although I have reckoned only twenty candles to be used in this statement, and have calculated therefore on the saving of twenty; yet twenty-five, or even more may be lighted, and at a very little additional expense, as it can only be in a little more coal being consumed; so that, if I were to reckon, that twenty-five or thirty candles were used, you see what a flattering advantage would appear. But I am not desirous of painting the picture with glaring colours, the saving in this statement is sufficient to satisfy every man, who is interested in it, of the great utility of the thing.

There is one thing more I would advance in its favour. It has been objected to by several, on occount of its disagreeable smell if any of the gas escape, or if the gas have not been properly purified. Now, Sir, the very thing they object to is an advantage. I would ask any medical man, whether the smell of burnt tar be injurious to the constitution. Why burn pitch and tar, to destroy contagion, if they have not the power of doing it? The gas will be found, instead of destroying health, to promote it, even in the last stages of consumption. It is a fact, the smoak of the coal fires in all large manufacturing towns, instead of injuring the health of the inhabitants, is good for it; for the contagious diseases are hardly ever known to appear, much less to increase in them. And how can we account for the prevention of these diseases, and for their not making sad havoc and devastation, in places, to all appearance so well calculated for their seat; crowded manufactories, crowded streets, filthy drunken workmen, the whole town at times quite enveloped with smoke; if we do not attribute it to its real and true cause, the coal smoke?

Its disagreeable smell not noxious,

but conducive to health.

The introduction of the gas into manufactories will be found still more useful and beneficial, as it will destroy the

inconvenience so often complained of in crowded shops and manufactories, where so many people are forced to breathe the same air over and over again, and which really is injurious to a very great degree. I say it will remedy this by purifying the air. I have no doubt it will completely hinder any thing like fever from spreading in those shops and manufactories, if it should be accidentally introduced. In fact it will be found to purge and purify the air, and to promote health, instead of injuring it.

I am, Sir,

Your humble servant,

B. COOK.

Caroline-street, Birmingham, Dec. 27th, 1808.

IX.

On the Superiority of Platina for making the Pendulum Spring of Watches. By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

Balance spring capable of improvement.

AS your valuable Journal is universally esteemed a source of the best information and most general utility to the mechanical as well as the philosophical world, I beg leave to trouble you to insert a few remarks on the subject of the pendulum spring, as it is a part of watch work I have often considered capable of the greatest improvement.

Platina the best material,

but arsenic must not be employed in consolidating it.

I have been long convinced of the superior advantages of platina over any other metal of which this instrument has hitherto been made, but I have not until lately been able to procure any of it in wire fit for that purpose. I find it by repeated experiments to possess great superiority on account of its inperceptible expansion; and, what is worthy of remark, the platina procured for this purpose should not be consolidated by arsenic, as it is by that means liable to expansion. I also find it, when properly drawn, to possess sufficient elasticity for any extent of vibration; it coils extremely well; and, if placed when coiled on the surface of a flat piece of metal, making one end of the spring fast, and marking exactly the other extremity, the slightest expansion

is not visible when applied to heat: so that if these springs are judiciously made of this metal, I am convinced they will turn out a general benefit to the public, which is the principal object of, Sir,

Your much obliged and obedient servant,

39, *Grafton-street, Dublin,* JAMES SCOTT.
June 20*th*, 1808.

P.S. Having for a length of time made use of this metal in my compensation curbs, I consider it as very superior to steel for every instrument of this kind. Superior to steel for compensation curbs.

N. B. This paper would have appeared much earlier, had it not been accidentally mislaid.

X.

On the Construction of Galvanic Batteries. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

EVERY lover of science in the country must have felt the great advantage of a philosophical Journal, in which he never fails to meet with a judicious selection of the most important discoveries, and whence he may hope to derive information, that would be vainly sought after in any regular treatise. Queries respecting the construction of galvanic batteries.

Some friends, who have united their efforts to follow Professor Davy in his grand experiments on the decomposition of the alkalis, would feel highly gratified by replies to the following questions.

What is the smallest number of Galvanic combinations, and the smallest surface of plates, that is sufficient to decompose the fixed alkalis? And, what is the best solution for charging a battery, so as to produce the greatest power?

We have seen, that batteries formed of the common rolled zinc do not act so powerfully as those, which have been made with zinc plates that were cast. It is said, that zinc is commonly alloyed, to make it roll the easier; but it is

is certain, that it may be rolled much thiner than is required for this purpose, in its pure state*.

I am, Sir,

Your obedient servant,

Jan. 2*d*, 1809. G. K. M.

REPLY.

Having taken the liberty of troubling Mr. Davy with a line on the subject here stated, I was favoured with his answer, from which I extract the following.

Battery capable of decomposing potash.

" In my early experiments upon potassium, I often procured it by means of a battery of one thousand pairs of plates of copper and zinc of six inches square, charged with a solution of concentrated nitrous acid in about forty parts of water. This is the lowest power that I employed: but as some of the plates had been much corroded by former processes, I should conceive, that a combination of eighty would be sufficient, provided the whole arrangement was perfect.

Less power sufficient for the earths and ammonia.

" The decomposition of the alkaline earths and ammonia by amalgamation or combination of their bases may be accomplished by a much weaker combination, fifty plates of six or four inches square being adequate to produce sensible results.

Potassium may be procured without electricity.

" The potassium, which I have used in various analytical enquiries lately carried on, has been all procured by chemical means, without the application of electricity.

" Potash may be decomposed by different processes, some of which are described in a paper, which I am now reading before the Royal Society; but the best method is that, which we owe to the ingenious researches of Messrs. Gay Lussac and Thenard, and which is the first of this kind, by mere chemical attraction, made known.

Chemical decomposition of potash.

" When melted potash is slowly brought into contact with iron turnings, or filings, heated to whiteness, hidrogen gas is evolved, holding potassium in solution: and if one part of the iron tube, or gun barrel, in which the experiment is made, be preserved cool, the metal is deposited in this part, being precipitated from the hidrogen gas by cooling.

* And at the common temperature. E.

" The

"The potassium is never procured quite so pure in this manner, as by electricity; but it is fit for analytical purposes, and I have obtained it with so little alloy, as to possess a specific gravity considerably below 8, water being 10. I have now by me a compact mass produced in an operation which weighs nearly 100 grains."

Near 100 grains in a mass procured by one operation.

XI.

Account of an economical Method of Evaporating the Water of Brinesprings, employed at the Salt Works of Moûtiers, in the Department of Mont-Blanc. By Mr. H. LELIVEC, *Engineer of Mines, &c. for the Departments of Mont-Blanc and the Leman Lake*.*

Brinesprings at Moûtiers.

THE richest spring at Moûtiers is constantly at the temperature of 30° R. [99·5° F.], and when cooled down to 10° [54·5°] indicates on the areometer 1·83°†. The poorest raises the thermometer to 25° [88° F.] only, and indicates 1·5° of saltness. The water is conveyed by troughs to a large reservoir, where it is left to settle; and thence it passes through other troughs to graduation houses about 1100 yards lower down. In its course it gives out bubbles of carbonic acid gas, and deposits a reddish sediment, which is at first oxide of iron, then a mixture of this with carbonate of lime, and at length almost wholly calcareous carbonate. It passes through four graduation houses in succession, and comes out of the last at the strength of 18°, and sometimes more. It is then boiled for about six and twenty hours, or till the salt begins to crystallize, keeping the boilers constantly full; a foulness, that rises, is scummed off; and the sulphate of lime it contains is precipitated.

Method of procuring the salt from them.

The sulphate of lime being raked out, in winter the evaporation is continued, with a slow fire, till the whole of the salt is deposited: but in summer a different process is followed, by which all the fuel consumed in the last stage of

* Journal des Mines, No. 120.

† The areometer commonly employed in the French salt works is divided into 30 degrees, beginning at distilled water, and ending at water saturated with salt.

the

New method of crystallizing the salt without fire.

the process is saved. When the solution is brought to the point of saturation, it is conveyed to a reservoir, whence it is raised by a chain-pump to a trough at the top of a wooden building, and extended its whole length. From this trough it runs into a series of very narrow troughs at right angles to it, and about two yards long. To each of these are twenty-five double or endless ropes, 6 millim. [2·4 lines] in diameter, 13 cent. [5 inches] from each other, and fixed 8 met. [26 feet] below. The saline water, flowing constantly out at notches cut in the sides of the troughs, trickles down the ropes, round which it forms a very thin coat, displaying a considerable surface to the solvent power of the air. As the water evaporates, the salt is deposited on the ropes. The water that flows down runs into the reservoir, and is pumped up again repeatedly, till it is exhausted, when it is suffered to run into the basin, that contains the mother-water.

The water of a fresh boiling is treated in the same manner, and thus seventeen boilings are raised in succession, forming one making, which occupies forty or five and forty days. At the expiration of this time the ropes are covered with a cylindrical coat of salt 7 or 8 cent. [2·75 or 3·15 inches] diameter, which is broken by a particular instrument for the purpose. As this process can be executed only in summer, seldom more than two making stake place in a year.

Quantity of salt made.

Every boiling, before it reaches this building deposits 100 myriagr. [2205 lbs] of salt in the boilers; and 650 myr. [14332 lbs.] are collected from the ropes, making in all 750 myr. [16537 lbs.] This process therefore does not yield quite so much salt, as the product of the evaporation of a similar quantity of water by two boilings would be 786 myr. [17332 lbs.]; but then there is a considerable saving of time and labour, as well as of fuel, and the salt obtained is more pure.

The process has been followed 20 years.

This process, equally ingenious and economical, invented by Mr. Dubutet, has been employed with success ever since the year 1788. It has not yet been adopted in any other salt works. It would be particularly advantageous in hot and dry climates.

It may be used

Mr. Roche has enhanced the utility of this building, by employing

employing it as a graduation house during the eight months in which it is not used for crystallization. He has found, that, all other circumstances being the same, the evaporation goes on nearly twice as fast in it as in the common graduation house with faggots. It is necessary however, that the brine should be of the strength of 4° or 5° before it is brought thither, otherwise the ropes would speedily rot.

as a graduation house,

and is much superior to the common one.

This building with ropes, the only one in existence, is 90 met. [295 feet] long, 17 met. [55¾ feet] of which are taken up by the pillars and the machine. It is divided into six arches, by party walls covered with boards; and each arch contains 40 troughs, and consequently 2000 single ropes 8·3 met. [27·2 feet] long; so that there are in all 12000 ropes, making a total length of 99600 met. [326546 feet]. Little expense is required for repairs, as three fourths of the ropes last seventeen or eighteen years from the time they are put up.

Dimensions of the building.

XII.

Eclipses of the Satellites of Jupiter, observed by JOHN GOLDINGHAM, *Esq. F. R. S., and under his Superintendance, at Madras, in the East Indies*.*

BETWEEN the beginning of the year 1794 and the end of the year 1802 Mr. Goldingham had 151 observations of eclipses of the first satellite of Jupiter, and 105 of the second satellite, either at their immersion, emersion, or both. Of these he has given tables, noting the day, whether immersion or emersion, apparent and mean time of observation, time by the Ephemeris, longitude of Madras by the tables, and circumstances of weather, &c. As the judicious remarks prefixed to these tables may be of use to future observers, it will not be unacceptable to several of our readers to find them here. They are as follows.

Eclipses of Jupiter's satellites.

The eclipses of the satellites of Jupiter were observed with achromatic telescopes, by Dollond, of three feet and half focal length, and magnifying power between 70 and 80;

Telescopes.

* Phil. Trans. for 1808, p. 322.

80; having been constructed more immediately for this purpose, for which they were exceedingly well calculated in all respects.

Clock.

An astronomical clock, with gridiron pendulum, and dead beat, regulated by transits of the sun and stars, was used for the time; which was deduced from the transit of the sun nearest the eclipse, and verified by the one immediately preceding or following.

Circumstances noted.

The circumstances under which the eclipses were observed are noted; from these may be inferred, how far the results are to be depended upon: those observed with the "air clear and the planet high" are the most satisfactory and valuable, nothing to the contrary being afterward expressed.

Longitude of Madras.

The longitude of the place, by numerous observations of various descriptions, is 5h : 21′ : 14″, or 80° : 18′ : 30″ east of Greenwich: by comparing this with the numbers in the last column, the difference of the tables will be obtained.

The greater number of these eclipses were not visible at Greenwich, but have been found very useful, when correspondent observations have been taken in India.

General remarks.

Persons not much in the habit of observing these eclipses, but desirous of obtaining as much correct information from their observations as possible, may find the following general remarks of use.

Correspondent observations necessary.

A correct difference of longitude, it would appear, is not to be expected, by comparing the time of observation with that in the tables; it therefore becomes necessary to have a correspondent observation to compare with, or some satisfactory observations taken under a known meridian, about the time; from which the errours of the tables may be found. Correspondent observations should, however, be obtained if possible: but it must not be supposed, that even these will give a correct difference of longitude, unless observed at both places, under the like favourable circumstances, and with telescopes of the same powers.

Most favourable circumstances.

The air being clear; the planet so high as to be out of the thick atmosphere, and to make the position easy; the telescope sheltered from the wind, and steady; neither moonlight nor twilight, and the satellite not near the body of the planet;

planet: An eclipse observed under such circumstances will, I apprehend, be as perfect as it well can be; and a correspondent observation, taken under the like circumstances, will give a correct difference of longitude of the two places, provided the eclipse be observed with telescopes of the same powers.

Much affected by unfavourable circumstances.

Taking the eclipses in the following tables observed under these favourable circumstances as the standard, and comparing their results, as given in the last column, with those of the others, it will be found, how much the latter are affected, by the eclipses having been observed when the atmosphere was hazy, or the planet very low, during twilight, or when the moon was near the planet, or the satellite not far from the body of Jupiter; and that even if correspondent observations had been taken, under favourable circumstances, at a known meridian, the difference of longitude given by the comparison would have been far from correct: the same eclipse observed at two places, under similar unfavourable circumstances, would possibly give a result near the truth; as the observations at both places would be affected in the same way, but probably not in an equal degree, as it is not likely there would be exactly the same degree of haze, the same strength of twilight, &c. at both places; and therefore those observations taken under the same *favourable* circumstances can only be relied upon with certainty.

Difficult to get telescopes alike.

It may not be an easy matter to have telescopes at both places of precisely the same powers for these observations: at Madras we had two telescopes in use, constructed at the same time, in appearance precisely alike, and intended by Dollond to have been so in all respects; yet on repeated trials, one was found to have a decided advantage of several seconds over the other, showing the emersions sooner, and the immersions later by that quantity. In order to do away the errour arising from a difference in the powers of telescopes, *immersions* and *emersions* should be observed at both places; the difference of longitude will be as much greater than it ought to be by one series as less by the other, but the medium will be the correct difference of longitude of the places: it is possible also there may be some difference in the

Both the immersion and emergion should be observed.

Difference in the eyes.

the eyes of observers, any errour which may arise from this source will also be done away by this method.

General requisites.

Hence it would appear, that, in order to obtain a correct difference of longitude of two places from correspondent eclipses of the satellites of Jupiter, the circumstances at both places should be similar and favourable; and that the telescopes should have equal powers, or that both immersions and emersions should be observed, which indeed ought always to be done, where time will admit: also, that, the circumstances being favourable at one place and not so at the other, a result very different from the truth will be obtained.

SCIENTIFIC NEWS.

Parkinson's Organic Remains.

THE first volume of Mr. Parkinson's Organic Remains of a former World, which I have noticed in Vol. IX, p. 143, has been some time before the public; and I have now the satisfaction of announcing the second, which is no way inferior to its predecessor. I trust it will be a subject of gratification rather than disappointment to my readers to find, that its indefatigable author has not been able to close his labours on the fossil remains of the animal world with the present volume, and that he cannot even pledge himself to limit them to a third; as he very judiciously means to render it as complete as possible, without admitting any superfluous matter. In his arrangement he nearly follows Wallerius, who, after treating of vegetables, proceeds through corals, worms, shells, insects, amphibia, fishes, birds, and quadrupeds up to man. The present volume is confined to zoophytes, many figures of which are given in twenty plates, executed with much elegance, and the greater part of them coloured after nature.

To enter into any adequate detail of the whole, or notice all the most striking parts, would much exceed my limits; I shall at present confine myself therefore to the following observations on madrepores.

Madrepores.

Under the genus madrepore are placed all those corals, the

the cavities of which are divided by lamellæ disposed in a stellular form. The animal, which in the recent coral fills these cavities, was first depicted by Donati in the 47th volume of the Philosophical Transactions, p. 105, Pl. IV, and in the Natural History of the Adriatic Sea by the same author. Its feet are in considerable number, and terminate externally in two conical productions, which, being placed on each side of every one of the lamellæ that give the stellular form to the cavity of the coral, serve to affix the animal to the circumference of its cell, and may with propriety be considered as the instruments, by which the little animal forms the lamellæ themselves. The bases of these conical productions unite and form round bodies, which possess somewhat of the figure and of the properties of a muscle: they undoubtedly serving to lengthen or shorten the feet, and also most probably to regulate the force, with which they clasp the lamellæ, on which they exert their plastic powers. The other ends of these round bodies terminate in small cylindric tubes, which are attached to the shell of the animal, in the centre of which is seen its head, capable of moving with great quickness, and ornamented with several rays, which are most probably the arms or claws with which it seizes and secures the animalculæ on which it feeds.

Manner in which the coral is formed.

Attributing the formation of these corals to the operations of the madreporean or medusean polype, let us endeavour to trace the little architect through its wonderful labours. Agreeable to the observations of Donati, each of the legs, as he terms them, of the polype are provided with two processes, which are applied to each side of one of the perpendicular laminæ, whilst a muscular pyriform body, attached to the other end of the leg, gives to it the power of employing that motion which is necessary for the accomplishment of its task. The young polype, disposed on an appropriate spot, may be considered as completing its operations by two distinct processes: the secretion and separation of carbonate of lime from the sea water, conveyed through the pyriform body; and its deposition, at its moment of secretion, by the two small processes, where the economy of the animal directs. Proportioned to the number of legs possessed by the infant animal was probably the number of perpendicular

lar laminæ, or pillars converging to the centre, which it began to erect; these when raised to a certain height, appearing to have been connected together by a horizontal plate of the same substance. On these the animal erected similar pillars, and placed on these a covering similar to that with which he had completed the first compartment. Thus seems to have proceeded the incessant labours of the minute artist: and as the number of its legs, or instruments, increased, and as they extended in length, so must the number of the perpendicular laminæ, and the circumference of the horizontal plates have also augmented. Thus must this curious fabric have derived its fashion from the growth and form of this minute and wonderful animal.

That the formation of these turbinated madrepores may have been thus effected does not appear difficult to conceive. Neither is it difficult to understand, that when the animal had attained its full extent of growth, the continuance of its labours would produce, not a body of a conical, but of a cylindrical form. Nor does it appear unlikely, that should any accidental circumstance change its horizontal position, a proportional deflection from the straight line would be occasioned; and a coralline body of a curved form be produced. Specimens of both these forms, it has been just remarked, are frequently found.

Reports of the Preventive Medical Institution.

THE long expected Reports of the Preventive Medical Institution at Bristol, have been left by the late Dr. Beddoes in some degree of forwardness. They will be completed and published as soon as possible, by Mr. King and Dr. Hook. The former of these gentlemen has been Surgeon to the Institution from its first commencement, and the latter has been connected with it since the month of March, 1804.

Life of Dr. Beddoes.

Dr. Hook is likewise about to publish a Life, &c. of Dr. Beddoes, with the approbation of his family and friends.

Commercial Magazine.

Mr. JOHN CLENNELL, late of Newcastle, has undertaken the editorship of the Tradesman, or Commercial Magazine.

Magazine. As this monthly publication, being of recent date, may be unknown to many of my readers, I shall just inform them, that its object is, to communicate general and useful information on the different subjects connected with trade, commerce, and manufactures.

St. Thomas's and Guy's Hospitals.

The spring courses of Lectures at these adjoining Hospitals will commence as usual the 1st of February, viz. Medical and surgical lectures.

At St. Thomas's.

Anatomy and Operations of Surgery, by Mr. CLINE and Mr. COOPER.

Principles and Practice of Surgery, by Mr. COOPER.

At Guy's.

Practice of Medicine, by Dr. BABINGTON and Dr. CURRY.

Chemistry, by Dr. BABINGTON, Dr. MARCET, and Mr. ALLEN.

Experimental Philosophy, by Mr. ALLEN.

Theory of Medicine and Materia Medica, by Dr. CURRY and Dr. CHOLMELEY.

Midwifery, and Diseases of Women and Children, by Dr. HAIGHTON.

Physiology, or Laws of the Animal Economy, by Dr. HAIGHTON.

Occasional Clinical Lectures on Select Medical Cases, by Dr. BABINGTON, Dr. CURRY, and Dr. MARCET.

Structure and Diseases of the Teeth, by Mr. FOX.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *Complete Course of Medical and Chirurgical Instructions.*

Errata in our last Number.

Page	line	
2	13	*For* E and B, *read* E A B.
	12 from bot.	*For* and $\frac{1.}{\text{rad.}}$, *read* varies inversely as radius.

METEOROLOGICAL JOURNAL

For JANUARY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

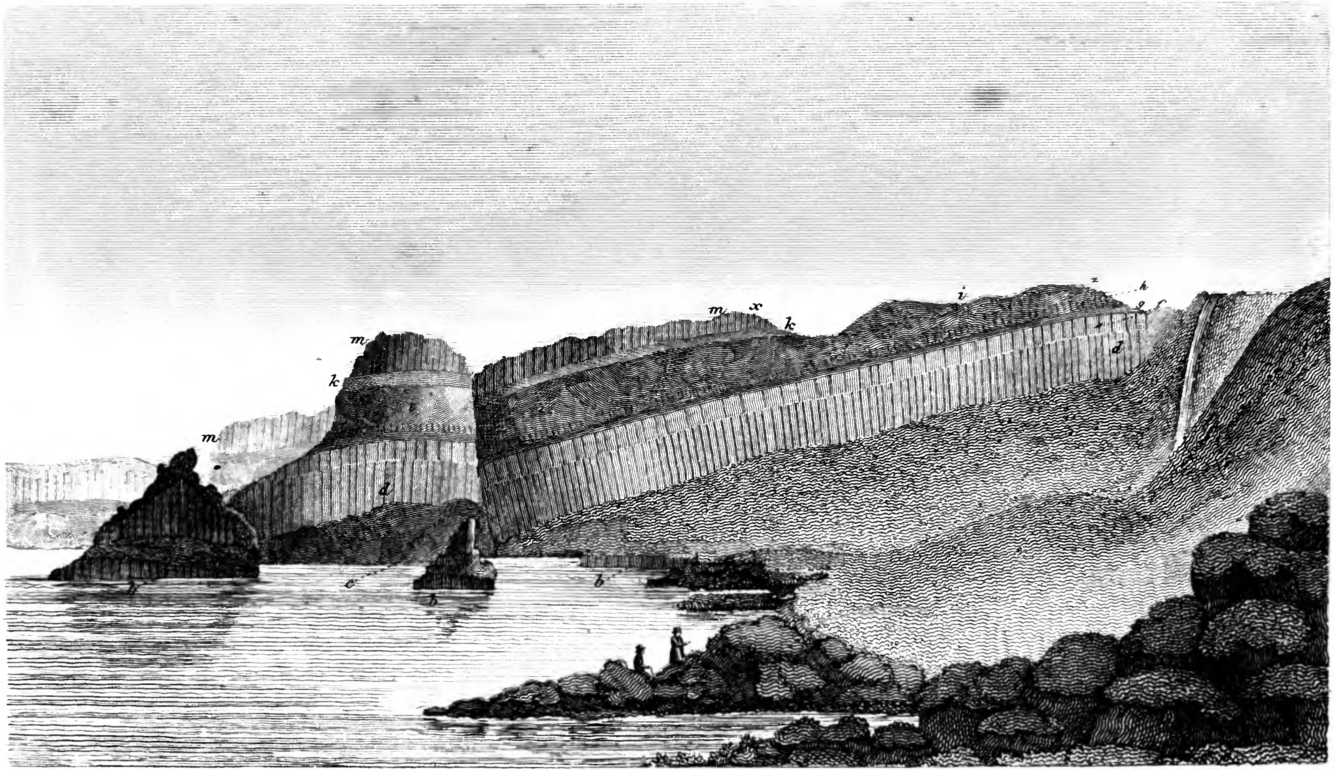
Day of DEC.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
27	32	33	34	30	29·44	Cloudy	Rain
28	34	37	39	35	29·58	Rain	Ditto
29	36	39	40	37	29·50	Ditto	Ditto
30	38	39	43	33	29·49	Ditto	Cloudy
31	34	38	40	35	29·59	Ditto	Rain
JAN.							
1	36	37	40	35	29·60	Ditto	Ditto
2	37	36	39	30	29·47	Ditto*	Ditto
3	31	30	32	28	29·33	Ditto	Snow
4	30	32	33	32	29·63	Ditto	Rain
5	37	43	44	42	29·67	Fair†	Ditto
6	42	42	44	38	29·50	Ditto	Cloudy
7	40	39	44	36	29·32	Ditto	Rain
8	40	40	42	36	28·82	Rain	Ditto
9	40	40	44	34	29·37	Fair	Ditto
10	38	39	43	36	29·20	Cloudy	Ditto
11	38	33	40	33	29·38	Ditto	Fair
12	35	32	36	31	29·49	Ditto	Ditto
13	33	34	38	28	29·69	Ditto	Rain
14	30	32	36	28	29·78	Ditto	Cloudy
15	30	28	30	26	29·75	Ditto	Snow
16	28	27	30	27	30·03	Ditto	Cloudy
17	28	27	28	21	30·06	Fair	Ditto
18	23	23	27	20	29·88	Ditto	Fair
19	24	29	30	29	29·72	Snow	Snow‡
20	28	30	32	25	29·48	Ditto	Foggy
21	32	32	33	29	29·46	Ditto	Cloudy
22	32	32	33	21	29·12	Ditto	Snow
23	22	31	33	30	29·67	Sleet §	Fair
24	32	33	40	33	29·40	Rain	Rain
25	36	33	39	32	29·43	Ditto	Cloudy
26	44	43	46	40	29·22	Cloudy	Fair

* Snow at 10 P. M. heavy snow all night.

† The constellation Orion brilliant. Very black in the north.

‡ At 1 P. M. appearance of change, rain freezing as it fell. The thermometers in a few minutes covered with ice, succeeded by rain and snow all the night.

§ At 11 heavy snow.



View of Portmoon.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1809.

ARTICLE I.

A Letter on the Alterations, that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry and Antrim. Addressed to HUMPHRY DAVY, *Esq. Sec. R.S. By* WILLIAM RICHARDSON, *D.D.**

SIR,

I Request you will be so good as to lay before the Royal Society the following observations on the Natural History of that part of Antrim, (contiguous to the Giant's Causeway,) which you and I examined so carefully together. I know not any country, that deserves so well to have its facts faithfully recorded; from the important conclusions to which they lead.

The basaltic country near the Giant's Causeway important to geology,

The basaltic area (taken in its whole extent) comprehends the greater part of Antrim, and the east side of Derry to a considerable depth.

* Philos. Trans. for 1808, p. 187.

and affords much facility to its study.

In a geological point of view, Nature* has been very kind to this district, for not content with assembling together in a small space so many of her curious productions, and arranging them with more regularity and steadiness than in any other country described, she has condescended occasionally to withdraw the veil, and lay herself open to view, often exhibiting a spectacle equally gratifying to the admirer of magnificence, and to the curious naturalist, who can here, by simple inspection, trace the arrangements, which are to be discovered elsewhere only by penetrating beneath the surface of the earth.

Particularly northward and on the coast.

As soon as we enter the basaltic area, we begin to perceive traces of these arrangements; as we advance farther north, they increase; and in the tract near the shore, and especially at the island of *Rathlin*, which seems to have come fresher from the hand of nature than the rest of our area, the stratification of the whole is perfectly visible, and the nature of the several strata laid open to us at their abrupt and precipitous terminations.

To the southward less distinct.

To the southward we perceive the distinctive features abate, and wear away; the basaltic stratification indeed remains, but is no longer displayed to us in the same manner; the neat, prismatic, internal construction of the strata, which occurs so frequently on and near the coast, is scarcely to be met with at a distance from it; a rude columnar appearance is all we find, and that but rarely.

O the north strata completely open.

It is at the periphery of our area, and especially at its northern side, that every thing is displayed to the greatest advantage; here we have perpendicular façades often continuous for miles, and every separate stratum completely open to examination.

Four most striking parts.

Of these façades, four are more distinguished by their grandeur and beauty than the rest, *Magilligan Rock*, *Cave Hill*, *Bengore*, and *Fairhead*.

The two former are at the extreme points of the north-west diagonal of our area, and nearly forty miles asunder;

* By the word Nature, which frequently occurs in the course of this Memoir, I always mean, according to Ray's definition, the wisdom of God in the creation of the world.

they

they are at the summits of mountains, and accessible by land.

The precipitous faces of *Fairhead* and *Bengore*, to which I had the pleasure of attending you, and which are visible only from the sea, are the most beautiful, and the most curious; for the strata, which at *Magilligan* and *Cave Hill* are all nearly similar, at *Fairhead* and *Bengore* are much diversified. Of *Fairhead* I have already published an account in Nicholson's Journal, for December, 1801, quarto series, and I now propose to execute an intention, which I have had for some years, of giving a minute account of *Bengore*.

Fairhead.

Bengore.

I am aware, that it will be extremely difficult to convey a clear and adequate idea of an assemblage of 16 strata, (for such is the number of which our promontory is composed), appearing and disappearing at various altitudes, yet retaining each its own proper place, and forming together a most beautiful and regular whole, though never considered as such before.

But as I have the aid of very correct views of the most important parts of the façade, to the accuracy and fidelity of which I have already obtained your testimony—I shall venture to proceed, for I am anxious to bring into notice the most complete exposure of the internal structure of a district, that I have seen or read of; as there is little likelihood that any other person will enjoy the opportunities, which I have had for so many years, of exploring this interesting part of our coast, through a turbulent sea, and rapid tides.

Description of the Promontory of Bengore, and its Stratification.

This promontory commences at the termination of *Bushfoot Strand*, where the coast, the general direction of which for several miles had been due east and west, turns to the north-east, and, after being cut into several semicircular bays, deflects to the S. S. E., and near the old castle of *Dunseverick* resumes its former rectilineal and nearly eastern direction.

Bengore described.

The

The promontory occupies the interval between *Dunseverick*, and the *Black Rock*, at the end of *Bushfoot Strand*, about four English miles; the façades commence at *Black Rock*, and increase in height until we reach *Pleskin*, where the perpendicular part at the summit is 170 feet, and the precipitous part from the bottom of the pillars to the sea 200. As we proceed on from *Pleskin* to *Dunseverick*, the height gradually abates, and is finally reduced to about 100 feet.

Cliffs.

Every slope covered with grass.

In this whole space, wherever the precipice is accurately perpendicular, the several strata are easily distinguished from each other, but where the slightest obliquity prevails, a grassy covering is formed, that effectually conceals all beneath it; hence the face of the precipice seems much diversified; the columnar strata in some places only exhibiting detached groups of pillars, while in others they form extensive colonnades.

General circumstances.

I shall now state the appearances as we approach and coast the promontory from the westward, noticing in this first view of the precipice every thing that may be considered as general, and reserving (as I did with you) for my return in the contrary direction, a detailed account of the strata taken separately.

The strata more inclined than the surface.

The first circumstance, that occurs to the attentive observer on his approach, is, that although both the promontory itself, and the strata composing it, ascend to the northward, yet it is not in the same angle, the strata being more inclined to the horizon than the line tracing the surface of the promontory, a fact which I shall account for afterward.

Strata.

From the *Black Rock* to the *Giant's Causeway* (about a mile) the materials, and their arrangement, are similar to those of the coast to the westward, viz. strata of table basalt, generally separated by thinner strata of a reddish substance.

New arrangement.

At the *Giant's Causeway* a new arrangement commences, one of the little systems I have mentioned in other memoirs, by the aggregate of which our coast is formed; nature having changed her materials, or their disposition, or both, every two or three miles. To the system of strata comprehended between

between the *Giant's Causeway* and *Dunseverick* I now limit myself, as all the strata composing it emerge between these two points.

Rise and fall of the strata.

As we proceed along the coast from the *Giant's Causeway* eastward, we perceive the whole mass of strata ascend gradually, culminate at the northern point of the promontory, and then descend more rapidly, as the land falls away to the south-east, until having traced them across the face of the precipice we see them immerge separately at and beyond *Portmoon whyn dikes.*

Whyn dikes.

The western side of the promontory is cut down perpendicularly, by eleven *whyn dikes*; the intervals between them are unequal, but they all reach from the top of the precipice to the water, out of which some of them again emerge in considerable fragments; they are all constructed of horizontal prisms, which are strongly contrasted with the vertical pillars of the strata through which they pass.

One of the dikes at *Port Cooan,* on *Bengore,* half a mile from the *Giant's Causeway,* is very beautiful; an insulated rock about 160 feet high, and 20 in diameter, stands perpendicular in the middle of a small bay; the main body of the rock is similar to the contiguous consolidated masses; but on the east side a singular whyn dike is joined to it, composed (as they often are) of several walls agglutinated together, with wall-like fragments of other parts of the dike emerging at their base; the solid mass of dike is seen cutting down the precipice to the southward at 150 yards distance.

Depressions of the Strata.

Depressions of the strata.

Soon after we have passed the last of our whyn dikes at *Port Spagna,* (a name derived from a vessel belonging to the Spanish Armada having been driven ashore in that creek), we discover a new and curious circumstance, viz. that the western half of the promontory has sunk or subsided between thirty and forty feet, without the slightest concussion or derangement of the parallelism of the strata.

Two other depressions appear as we proceed onwards, one at *Portmoon,* and the other at the angle where the promontory begins to project from the rectilineal coast; these however

however are far less considerable in thickness than the preceding, neither of them exceeding five feet.

Such depressions occur at the collieries near *Ballycastle*, and generally on one side of a whyn dike. We have also at *Seaport*, two miles west from the *Giant's Causeway*, a dike, oblique and undulating, with a depression of the strata of about four feet on one side; but on *Bengore* promontory our dikes are unaccompanied by depressions of the strata, and where we have depressions, we do not find a trace of a dyke.

The portions of this extensive façade, which I have selected for explanatory views, are *Portmoon*, in or near which most of the strata emerge, and *Pleskin*, where the strata culminate, each of these views too exhibits one of our depressions, but in that of *Pleskin*, the first apparent depression is purely an optical effect arising from the position of my friend Major O'Neal, of the 56th, who took his view from the water.

Enumeration of the sixteen Strata that compose the Promontory of Bengore, taken in their regular Order, and counting from above.

Enumeration of the strata.

The country immediately to the southward of *Bengore* is like the Promontory itself a stratified mass, accumulated to the summits of *Craigh Park* and *Croaghmore*, the first five hundred and the second seven hundred feet high; but with these strata I have nothing to do, limiting myself to those alone of which the promontory is formed, and which are exhibited in its façades.

The uppermost of these commences near half a mile to the eastward of the angle, where the coast, deflecting from its due east and west course, turns to the north-west, and begins to form the promontory.

So far the course of this stratum is to appearance perfectly horizontal, for the strata all ascending to the north, the intersection of their planes with the plane of the sea must run east and west, that is, in the present case it coincides with the direction of the coast.

But when the coast changes its direction, this coincidence ceases, and the façade (that is the vertical section of the coast)

coast) losing its east and west course, its strata must appear to ascend towards the point it turns to; therefore the strata at *Portmoon*, and along the north-east side of the promontory, should ascend obliquely along the façades, as they actually do.

First Stratum, (*m*). See Pl. V.

The stratum I commence with forms the whole façade, from its first appearance until it reaches the promontory; it consists of massive pillars rather rude, and about sixty feet long, its course for half a mile (as I have stated) seems horizontal, but on the face of the promontory it ascends, and continues to rise uniformly until it reaches the summit, which it lines as far as *Portmoon*, on the south side of which it loses some of its thickness, then suddenly disappears and vanishes from that façade, receding westward in the form of a stony ridge, and is seen no more. 1st stratum.

Second Stratum, (*k*).

The stratum upon which the preceding rests is red as brick, and about nine feet thick; it appears in spots and patches just above high water mark, so long as the incumbent stratum continues horizontal, but when that rises obliquely, the second ascends with it; it is now completely displayed, and having supported the preceding in its course to the summit, vanishes with it (at *x* in the view of *Portmoon*,) and is seen no more. 2d stratum.

These ochreous matters, so common in all basaltic countries, according to Mr. F. St. Fond's opinion, were once pure basalt, but have undergone some chemical process of nature we are unacquainted with, by which their colour has been changed.

Third Stratum, (*i*).

The next stratum is the last of those composing the promontory which appears beyond it; for so long as the first and second continue their horizontal course toward *Bengore*, this third accompanies them, showing its upper surface between high and low water mark; but when it ascends along 3d stratum.

along with the others across the façades, it displays its whole thickness, above fifty feet.

Irregular prismatic basalt. This stratum is of that variety of basalt I have on different occasions distinguished by the name irregular prismatic; it resembles the columnar basalt in grain, but differs from it totally in principle of internal construction, for its prisms are small, not articulated, and indifferent as to the position of their axes, which is perpetually changing.

The irregular prismatic basalt accompanies the columnar in most countries, as at *Pont du Baume*, at *Trezza*, at *Bolsena* in the *Sound of Mull*, and at *Staffa*. In *Antrim* it is very common; and here is a striking resemblance between the rock crowning the celebrated columns at *Staffa*, and a stratum covering a very neat colonnade at *Craigahullur*, near *Portrush*.

This stratum (as is well exhibited in the view of *Portmoon)* is scolloped off irregularly from the point where it becomes superficial (*x*), until it completely disappears at (*z*); a thin stripe of its lower edge alone is ever resumed again.

Fourth Stratum, (*h*).

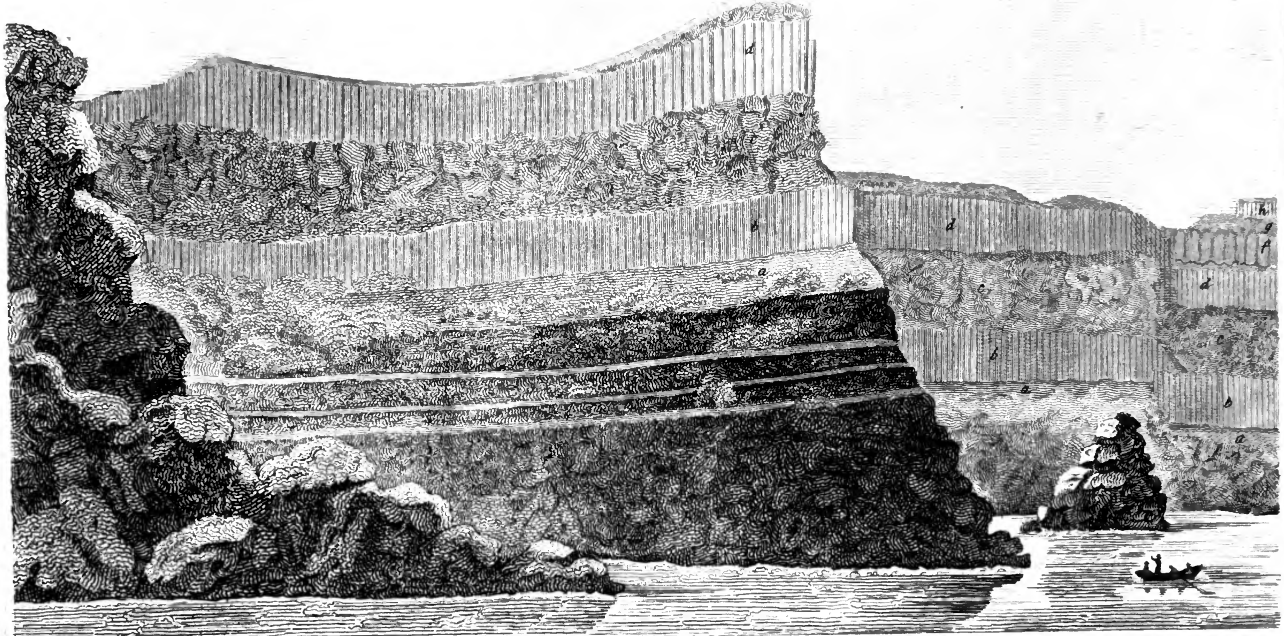
4th stratum. The next three strata will require only very short descriptions; the fourth is about seven feet thick, entirely columnar, the pillars small, but not neat; they appear very white from a thick covering of *byssus saxatilis*, which shows a great predilection for this stratum.

Fifth Stratum, (*g*).

5th stratum. This stratum is ochreous, and more of a slate colour than any of the other red strata; as it is friable, it soon acquires a grassy coat, through which it is only in spots that it shows its proper colour; it is about eight feet thick.

Sixth Stratum, (*f*).

6th stratum. This stratum is composed of rude massive pillars so coarsely formed, that on the least abatement of perpendicularity the columnar form can scarcely be traced. This stratum is about ten feet thick, it forms the vertex of the beautiful



Ruffe sc. 52 Queen Ann Street East

View of Pleskin on the North West side of Bengore Promontory.

beautiful conical island *Beanyn Daana,* and is marked in the views (*f*)*.

These last strata, though they have nothing very remarkable in themselves, nor contribute much to the beauty of the façade; yet they exhibit one of the most important facts I am acquainted with in natural history, and which, when attentively considered, throws much light on the nature of the operations performed upon our globe since its consolidation, and leads us irresistibly to conclusions extraordinary and unexpected. These lead to important facts.

The fourth, fifth, and sixth strata reach the top of the precipice, and vanish together at the waterfall in the north-west corner of *Portmoon.* When they come to the surface, they turn inland to the westward in long stony ridges; these obstruct the course of the waters in their descent along the inclined plane, formed by the surface of the promontory, and throw them over the precipice in a cascade highly beautiful after rain.

On the façades to the north-west not a trace of them appears, these being entirely formed by the lower strata, which I have not yet noticed; but at the distance of a mile, at the great depression (already mentioned), the fourth, fifth, and sixth strata, with a narrow stripe of the third, suddenly appear, in their regular posts, their proper order, and with all the characteristic marks peculiar to each separate stratum.

In the interval between the depression at *Pleskin,* and the *Giant's Causeway* (about a mile), these three strata often appear in a desultory way on the summit of the precipice, wherever it is of sufficient height to receive them, always preserving their usual thickness, their characters, and their order: so that a person master of the order I am detailing, as he approaches a rising point of the precipice, can tell its strata, and their order, before he is near enough to distinguish them.

Seventh Stratum, (*d*).

The rude and massive pillars of the sixth stratum pass into the neater and much longer columns of the seventh, 7th stratum.

* The view of Pleskin will be given in our next.

without

without interrupting the solidity or continuity of the material; exactly as a down held hand appears to separate into fingers. The thickness of this stratum, that is the length of the pillars of which it is formed, is fifty-four feet; it is marked (*d*) in the two views, and in its passage across the face of the precipice displays more beautiful colonnades than any of the others.

This seventh stratum emerges from the beach immediately behind the south-east point of *Portmoon*, and where it first shows itself in that bay has its lower edge raised only a few feet above the water; it forms the upper frustum of the larger of the two conical islands, ascends obliquely along the face of *Portmoon*, and continues to rise until it composes the upper range in the beautiful façade, properly called *Bengore Head*. This is properly the most magnificent of all, its convexity towards the sea producing a fine effect. The lower edge of this stratum, that is the line forming the base of its pillars, has here, as at *Pleskin*, attained the height of three hundred feet above the water.

This seventh stratum, like those above it, also suffers an interruption; for after having exhibited itself to such great advantage at *Bengore*, the extreme northern point of the promontory lowers, and this stratum disappears for about one third of a mile; as the promontory rises, it is resumed again in great beauty at *Pleskin*, and is interrupted no more; we scarcely ever lose sight of it until we reach *Port Noffer* (the next bay to the Causeway); here, for want of perpendicularity it is little seen, and is finally lost over the Causeway, we know not well how.

Disappears: is resumed again: and at last vanishes.

Eighth Stratum, (e).

8th stratum.

The next stratum is of the same variety of basalt with the third, that is, irregular prismatic; it is fifty-four feet thick, and in the views distinguished by the letter (*c*): where it emerges at the south-east corner of *Portmoon*, it is quite accessible by land, and affords the best opportunity I know for examining this species of basalt, as it is there very neat.

There is little more of this stratum seen in the façade of *Portmoon* for want of perpendicularity, but it forms the lower

lower frustum of the great conical island *Beanyn Daana*, and the whole of the smaller, except the base; it is well displayed over the remainder of the precipice, it forms the intermediate stratum between the magnificent colonnades at both *Bengore* and *Pleskin*, and finally is lost just over the *Giant's Causeway*. Large globular fragments have fallen from it, and are scattered about the causeway. Beanyn Daana.

Ninth Stratum, (b).

This stratum is forty-four feet thick, that being the exact length of the neat pillars composing it; at its emersion it forms the bases of the two conical islands in *Portmoon*, and is no more seen in that bay, but immediately to the northward it begins to show itself in colonnades and groups, some of them resembling castles and towers. 9th stratum.

It ascends along the precipice obliquely, like those above it, forms the lower range at *Bengore* and *Pleskin*, from which last it dips to the westward regularly, composes the group at *Port Noffer*, called the *Organs*, seen from the causeway, and finally at its immersion, or intersection with the plane of the sea, it forms the beautiful assemblage of neat pillars, so long distinguished by the name of the *Giant's Causeway*. The Organs.

At these two intersections, each of them accessible by land and water, the prisms exactly resemble each other in grain, size, and neatness; the interval between them is full two miles, through great part of which this stratum is displayed at different heights; it culminates between *Pleskin* and *Bengore*, with its lower edge more than two hundred feet above the water.

We see now what a diminutive portion of our vast basaltic mass has, until lately, monopolized the attention of the curious; and even after it was discovered, that we had many other, and much finer collections of pillars on the same promontory, it never occurred to those who were preparing to give accounts of them to the public, to examine whether these were mere desultory groups, or detached parts of a grand and regular whole, which a more comprehensive view of the subject would soon have laid open to them.

Tenth

Tenth Stratum, (a).

10th stratum. The stratum upon which the pillars of the preceding rest is ochreous, red as minium, and about twenty feet thick; it is scarcely seen at *Portmoon*, a patch alone of its surface being distinguishable under water at low tide; but immediately to the northward it shows itself, and from its bright colour makes a conspicuous figure across the face of the precipice in a course of more than a mile and a half; its last appearance to the westward is at *Rovinvalley*, the opposite point of the bay from the *Giant's Causeway*, from which we have a good view of it. The final dip and immersion of this tenth stratum, as well as its emersion, are lost for want of perpendicularity.

Rovinvalley.

11th to 16th strata. The six remaining strata are all similar in material, but differing much from each other in thickness; they are all of that description called tabular basalt, sometimes showing a faint disposition to assume a columnar form at their edges, and always separated from each other by ochreous layers.

Not so distinct as the others. These six strata are not so perfectly distinct as those above them, for sometimes we think we can count seven, and again not more than five; nor does each of these preserve the same thickness through their whole extent, for they are deeper towards the northern point, where they culminate, forming by themselves a perpendicular façade near two hundred feet high, but they grow thinner as they recede from this centre.

The jets of black rock in the view of *Portmoon* are the emersions of these strata; their last appearance on the west side is at *Rovinvalley*, where they strongly display the inclination of their strata (the same with all the rest) to those approaching from the westward; their final immersion is lost for want of perpendicularity.

I shall now proceed to select from the great mass of facts, that are exhibited on the face of *Bengore* promontory, and occur in the contiguous basaltic country, such as seem applicable to geological questions, and likely to throw light on such subjects.

Facts

Facts applicable to geological Questions.

Geological facts.

1. Every stratum preserves accurately, or very nearly, the same thickness through its whole extent, with very few exceptions.

The strata uniform in thickness.

2. The upper and lower surface of each stratum preserve an exact parallelism, so long as they are covered by another stratum; but when any stratum becomes the superficial one, its upper surface is scolloped, or sloped away irregularly, while the plane forming its base continues steady, and rectilineal; but the parallelism of its planes is resumed as soon as another stratum is placed over it.

Both surfaces parallel, unless one has been exposed to the air.

3. The superficial lines bounding the summit of our façades, and our surface itself, are unconnected with, and unaffected by, the arrangement of the strata below them.

The line of the surface not governed by the strata below.

4. Nature, in the formation of her arrangements, has never acted upon an extensive scale in our basaltic area, (at least on its northern side, where our continuous precipices enable us to determine the point with precision,) but changes her materials, or her arrangement, or both, every two or three miles, and often at much smaller intervals.

Materials and arrangement frequently changed.

5. Wherever there is a change of material, as from one stratum to another in a vertical line; or where the change is in a horizontal direction by the introduction of a new system; or where a whyn dike cuts through an accumulation of strata; in all these cases the change is always *per saltum* and never *per gradus*, the lines of demarcation always distinct, and well defined; yet the different materials pass into each other without interrupting the solidity and continuity of the whole mass.

The changes always sudden, without interruption of continuity.

6. The façades on our coast are formed as it were by vertical planes, cutting down, occasionally, the accumulations of our strata; the upper part of these façades is generally perpendicular, the lower steep and precipitous.

Precipices.

7. The bases of our precipices commonly extend a considerable way into the sea; between the water and the foot of the precipice, (and especially near the latter) there is frequently exhibited the wildest and most irregular scene of confusion, by careless observers supposed to be formed by the ruins of the precipice above, which have fallen down; such,

Their bases extend into the sea.

Apparent fragments on them

not fallen down from above.

such, no doubt, was Mr. Whitehurst's idea, when he describes one of these scenes as "an awful wreck of the terraqueous globe."

But a more attentive observer will soon discover, that these capricious irregularities, whether in the form of rude cones, as at *Beanyn Daana*, and the west side of *Pleskin;* or towers, as at the dike of *Port Cooan*, and *Castro Levit*, at the foot of *Magilligan* façade; or even spires and obelisks, as to the westward of *Kenbaan*, and at the *Bull of Rathlin;* yet all of these once formed part of the original mass of coast, stratified like it, and their strata still correspond in material and inclination with those in the contiguous precipice.

Perpendicular cliffs not confined to the coast.

8. These vertical sections or abruptions of our strata are by no means confined to the steeps that line our coast; the remaining boundary of our basaltic area has several of them equally grand; and similar abruptions, or sections (though not so deep) are scattered over a great part of our area, and especially on the ridges of our hills and mountains, which are cut down in many places like a stair by the sudden abruption of the basaltic stratum.

The materials on one side of these carried away.

9. Wherever the strata are thus suddenly cut off, whether it be a mass of accumulated strata, as in the façades on our coast, or solitary strata, as in the interior; the materials on one side of the abruption are completely carried away, without a fragment being left behind, while on its other side the untouched stratum remains intire and undisturbed.

I shall not proceed to apply these facts to support, or invalidate, any of the numerous theories, which have given rise to so much controversy, in which I myself (as you know) have borne some part; I shall look to nature alone, without much reference to opinions, and shall endeavour to trace, by the marks she has left behind her, some of the grand operations she once executed on the surface of our globe.

The 3 divisions of history applicable to natural history.

Varro divided the time elapsed since the beginning of the world into three portions, which he distinguished by the names, *prolepticum*, *fabulosum*, and *historicum*.

The first comprehended the period of absolute darkness; in

in the second some faint lights were thrown upon the history of its events by fable and tradition; in the third, the historian had the common aids, from which history is usually compiled.

The natural history of the world seems to admit of a corresponding division. In the first I include the formation of our strata, their induration, their derangement from the horizontal position in which they seem originally to have been placed, and the operation of cutting them down by so many whyn dikes. Period of absolute darkness.

In the second division, corresponding to Varro's *fabulosum*, I comprehend the operations performed upon our globe posterior to its final consolidation, and antecedent to all history or tradition; operations therefore that can be established by the visible effects alone which still exist, written in strong characters. Fabulous period.

The third division contains the period since we acquired some knowledge of natural history, became acquainted with causes and effects, and able to trace the connection between them. Historical period.

With the operations performed in the first division (corresponding with Varro's *prolepticum)* modern theorists assume that they are well acquainted, able to account for every appearance, and to detail the whole process of original formation. I however shall decline noticing these early processes of nature, and limit myself to the second division of natural history, hoping from the prominent features of my country that remain still undefaced, and from its curious facts, to trace and demonstrate the great effects, that have been produced upon our surface; and though I do not presume to advance farther, I perhaps may assist in clearing the way for future naturalists, and by establishing effects, encourage them to proceed to causes, and help them to discover the powers and agents, by which these grand operations have been executed. Modern theorists.

(To be concluded in our next.)

II.

On the Purification of Camphor by Means of Potash. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Camphor purified by pure potash.

FROM an idea of pure potash having a greater affinity for fixed oils, than the essential ones, and considering camphor as one of the latter in a concrete state, I was induced to try its effects on some, which, though not the unrefined camphor of commerce, was very impure, and possessed considerable empyreumatic smell. After several experiments with mixtures of it, different fixed oils, and sand (in order to divide the particles), I found, that, when sublimed with a small admixture of pure potash, the oily particles and empyreumatic smell were detained by the alkali. The subcarbonate does not answer the purpose, because in that state the affinity of potash for oils is less than when entirely deprived of carbonic acid.

Subcarbonate ineffectual.

6 p. camphor, 3 olive oil, 24 sand, and 1 potash, mixed, and the camphor sublimed.

Two drachms of camphor with considerable empyreumatic smell, and dirty, were mixed with one of olive oil, and eight of sand; after which twenty grains of pure potash were added, and heat applied; but though it was greater than is necessary for its sublimation, the product was perfectly free from empyreumatic smell, and a little whiter than it generally is.

Substituting linseed oil produced no alteration in the product; and supposing, that the fixed oily particles of camphor are not more liable to render it empyreumatic than those employed, which did without the addition of the alkali, I take the liberty of submitting this to your perusal, and am,

Sir, yours, &c.

PHILOCHEMICUS.

Jan. 4, 1809.

III.

III.

An Account of some Peculiarities in the anatomical Structure of the Wombat. By EVARARD HOME, *Esq. F. R. S**.

A Male wombat was brought from the islands of Basse's Straits, by Mr. Brown, the naturalist attached to Captain Flinders's voyage of discovery. It was entrusted to my care, and lived in a domesticated state for two years, which gave me opportunities of attending to its habits.

Wombat from Basse's Straits.

It burrowed in the ground whenever it had an opportunity, and covered itself in the earth with surprising quickness. It was quiet during the day, but constantly in motion in the night: was very sensible to cold; ate all kinds of vegetables; but was particularly fond of new hay, which it ate stalk by stalk, taking it into its mouth like a beaver, by small bits at a time. It was not wanting in intelligence, and appeared attached to those to whom it was accustomed, and who were kind to it. When it saw them, it would put up its fore paws on the knee, and when taken up would sleep in the lap. It allowed children to pull and carry it about, and when it bit them did not appear to do it in anger or with violence. It appeared to have arrived at its full growth, weighed about twenty pounds, and was about two feet two inches long.

Its habits.

The koala is another species of the wombat, which partakes of its peculiarities. The following account of it was sent to me some years ago by Lieut. Colonel Paterson, Lieutenant-Governor of New South Wales. The natives call it the koala wombat; it inhabits the forests of New Holland, about fifty or sixty miles to the south-west of Port Jackson, and was first brought to Port Jackson in August, 1803. It is commonly about two feet long and one high, in the girth about one foot and a half; it is covered with fine soft fur, lead coloured on the back, and white on the belly. The ears are short, erect, and pointed; the eyes generally

The koala another species.

* Abridged from the Philos, Trans. for 1808, p. 304.

ruminating, sometimes fiery and menacing; it bears no small resemblance to the bear in the fore part of its body; it has no tail; its posture for the most part is sitting.

Hunted by the New-Hollanders.

The New Hollanders eat the flesh of this animal, and therefore readily join in the pursuit of it; they examine with wonderful rapidity and minuteness the branches of the loftiest gum trees; upon discovering the koala, they climb the tree in which it is seen with as much ease and expedition as a European would mount a tolerably high ladder. Having reached the branches, which are sometimes forty or fifty feet from the ground, they follow the animal to the extremity of a bough, and either kill it with the tomahawk, or take it alive. The koala feeds upon the tender shoots of the blue gum tree, being more particularly fond of this than of any other food; it rests during the day on the tops of these trees, feeding at its ease, or sleeping. In the night it descends and prowls about, scratching up the ground in search of some particular roots; it seems to creep rather than walk: when incensed or hungry, it utters a long shrill yell, and assumes a fierce and menacing look. They are found in pairs, and the young is carried by the mother on its shoulders. This animal appears soon to form an attachment to the person who feeds it.

Its habits.

A specimen of this animal has since been sent to me in spirits; the viscera had been removed, but the male organs of generation, and the structure of the limbs, were the same as in the wombat. There was no subdivision of vessels in the groin as in the tardigrade animals.

The wombat has been in part described.

The external form of the wombat has been described by Mr. Geoffroy in the second volume of the Annales du Museum National de France; and several parts of its internal structure have been taken notice of by Mr. Cuvier in his Leçons d'Anatomie comparée. It only remains to mention such peculiarities as have either been slightly touched upon, or entirely passed over in the different accounts. Among these is the mechanism of the bones and muscles of the hind legs, which differs in many respects from that of all other animals, except the koala. The following account of it is drawn up at my desire by Mr. Brodie, from an accurate examination of the parts.

Peculiarities of its hind legs.

" There

" There is no patella; but the tendon of the extensor muscles of the leg, where that bone is usually situate, is much thickened.

" The fibula is proportionably larger than in most animals. At the upper extremity it is broad, and has two distinct articulating surfaces: the anterior of which is articulated to the tibia, and the posterior to a small bone of a pyramidal shape, which is connected to the tendon of the external head of the gastrocnemius muscle like a sesamoid bone. The lower extremity of the fibula is large, and forms about half of the articulating surface for receiving the tarsus at the ankle. An interarticular cartilage is here interposed between the tibia and the fibula, and another between the fibula and the tarsus.

" The fibula has a slight degree of motion on the tibia at its upper end, and a half rotatory motion on it at its lower end. Between the two bones is a strong muscle, which passes from one to the other throughout their whole length. The fibres have their origin from the inner edge of the fibula, and pass obliquely inward and downward to be inserted into the opposite surface of the tibia. When this muscle contracts, it pulls the fibula forwards, and produces a degree of rotation on the tibia, which turns the toes inwards. The anterior surface of the muscle is covered by a thin fascia or interosseous ligament, and there is another fascia less complete on its posterior surface. The muscle of the legs, corresponding to the biceps flexor of the human subject, is inserted into the posterior part of the fibula, and is an antagonist to the muscle just described. Its action brings the toes back to a straight line, but does not turn them outwards."

This structure common to two animals of different habits.

This mechanism is met with in two animals, whose mode of life is very different, the one living on trees, the other not; but as they both burrow in the ground during the night, its use appears to be for throwing back the earth while the animal is burrowing. There is nothing at all similar to it in the hind legs of the mole, or other burrowing animals.

Stomach.

The internal structure of the stomach of the wombat resembles very closely that of the beaver. This is so different from that of the kangaroo, and all the other animals of the opossum tribe, that it forms a very extraordinary peculiarity.

IV.

On the Changes produced in Atmospheric Air, and Oxigen Gas, by Respiration. By W. ALLEN, *Esq., F. R. S., and* W. H. PEPYS, *Esq., F. R. S*[*].

Respiration has been investigated

THE process of respiration, or breathing, is so intimately connected with our existence in life, that from its first moments to the final close, sleeping and waking, this necessary action is constantly maintained: nor can it be suspended even for a few minutes without considerable pain and the utmost danger. This important process has of course excited the curiosity both of ancient and modern philosophers; among the latter we find the distinguished names of Mayow,

by many,

Priestley, Goodwin, Menzies, Spallanzani, Scheele, Lavoisier, and Davy, whose successive labours have thrown great light upon this difficult subject, and prepared the way for farther investigation; but it is impossible to take a review of what has already been done, without perceiving, that some

but some important points still unsettled.

important points were by no means satisfactorily settled; an accurate method of separating the different gasses, and ascertaining their exact proportion in any given mixture, was still a desideratum when many of the experiments were made; and it is only of late years that eudiometry has at-

Residual gas in the lungs often a difficulty.

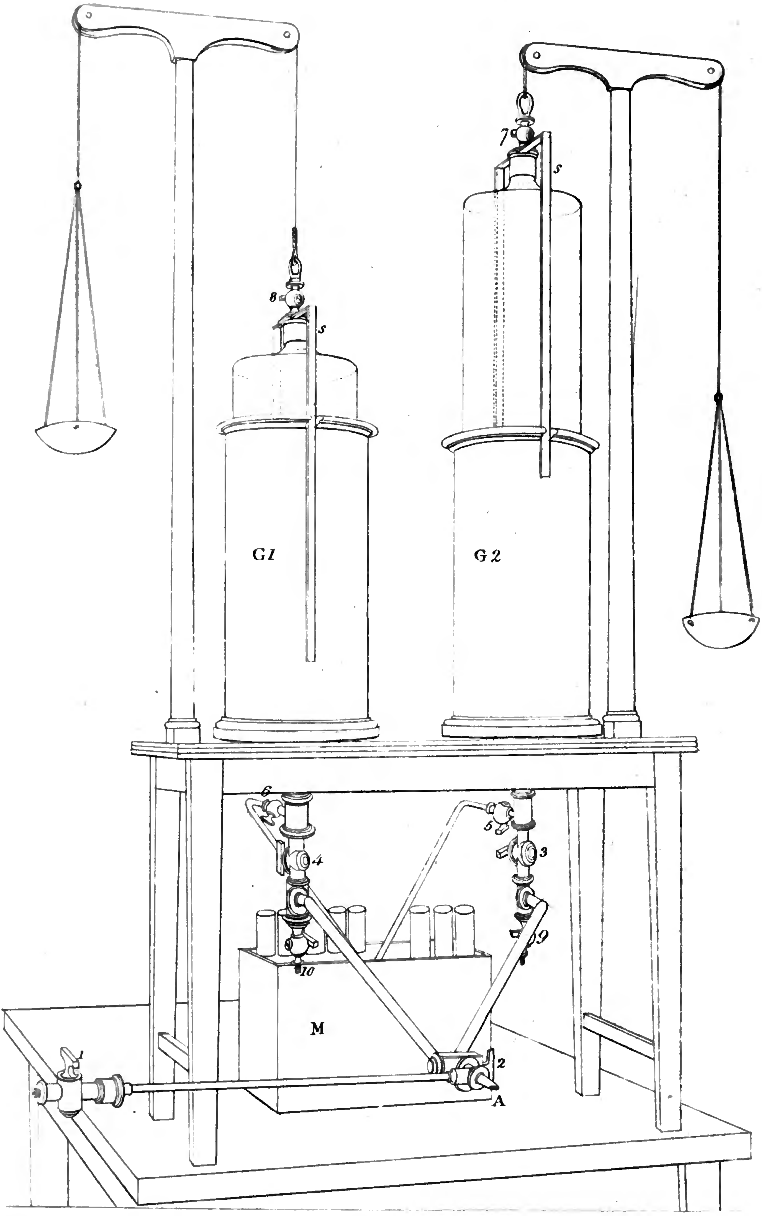
tained its present perfection. The quantity of residual gas in the lungs after a forced expiration was a matter in dispute among former experimenters, some making it one hundred and nine cubic inches, and others only forty; and yet it is of the utmost importance in all calculations upon the effects produced, especially upon small portions of gas, that the state of the lungs should be accurately determined; this constitutes the great difficulty in the investigations. We

This obviated.

therefore commenced our labours by constructing an apparatus, in which we are able to respire from three to four thousand cubic inches of gas, conceiving, that in this quantity, the errour arising from the residul gas in the lungs must be so much obviated as to permit the most satisfactory results.

[*] Philos. Trans. for 1808, p. 249.

The



Mess.rs Allen & Pepys's Apparatus for showing the changes produced in Air by respiration

The apparatus consists of three gasometers, two of which are filled with mercury, and one with distilled water. Apparatus for the purpose.

The water gasometer which belongs to the Royal Institution is capable of holding four thousand two hundred cubic inches of gas, and each of the mercurial ones three hundred cubic inches; the apparatus was so arranged, that the inspirations were all made from the water gasometer, and the expirations into the mercurial gasometers alternately. Each of the gasometers is furnished with a graduated scale, and they are all made to range with each other, so that the quantity of gas inspired and expired could be immediately and exactly ascertained: to each of the mercurial gasometers a glass tube is fixed, and made to enter a mercurial bath, from which portions of the expired air could at any time be taken for examination.

By the kindness of our friend Silvanus Bevan, we are enabled to give an accurate drawing of the apparatus.

Description,

Pl. VI, fig. 1. The communication with the water gasometer. Reference to the plate.

2. A cock so constructed that it might be made to communicate with either of the mercurial gasometers, while at the same time all connexion with the other was cut off.

A. The mouth piece.

3 to 10. Brass cocks.

G. 1, and G. 2. Mercurial gasometers.

S. S. Scales graduated to cubic inches.

M. Mercurial bath.

The large reservoir or water gasometer is not shown in this drawing, it having been so frequently described in chemical works.

Manner of conducting the Experiment.

Our first care was, to be certain that all the parts of our apparatus were perfectly air-tight, and this, from the nature of it, was very easily ascertained; we agreed, that the breathing should always be performed by one of us, and the registering &c. by the other, as each would by these means acquire Manner of conducting the experiments,

quire a greater degree of dexterity in performing his part, and the results would be more uniform.

Experiment. The water gasometer being filled with common air to a certain mark upon the scale, and the mercurial ones completely empty, the person to breathe, whom we shall uniformly call the operator, was seated upon a stool, with his mouth even with the tube A, his nose being secured with a steel clip. He made as complete an expiration as possible into the open air, then applying his lips to the tube, and keeping his left hand constantly on the cock 1, and his right hand on the cock 2, he opened the communication with the water gasometer, and made an inspiration; then immediately closing it, he opened with his right hand the cock at 2; and that at 4 being also opened, he expired into the mercurial gasometer G 1; then closing 2, which cut off all communication with the mercurial gasometer, he opened 1, in order to make a fresh inspiration; then closing it, he again opened 2, and expired into the mercurial gasometer; and proceeding in this way, always taking care to shut one cock before the other was opened, the air was made to pass from the water gasometer, through the lungs of the operator into the mercurial gasometer, and this with great ease, as the diameters of the tubes were purposely made large. The scale of the mercurial gasometer was carefully noticed, and when nearly full, the cock 4 underneath was shut off: then, by a signal from the operator, his colleague opened 3, and the expirations were received in G 2. While this was filling, the number of cubic inches in G 1 was registered, a portion saved in the mercurial bath, and the rest quickly expelled. This operation was repeated, until the contents of about twelve or thirteen mercurial gasometers were taken off: the operator always concluding with a strong effort to empty his lungs as completely as possible. The quantity inspired from tha water gasometer was then compared with the quantities expired into the mercurial gasometers, and the difference noted. The following are the results of the first ten experiments.

No.

Results of the 1st ten experiments.

No.	Time.	Cubic inches of common air inspired.	Cubic inches of gas expired.	Deficiency.
1.	time not noted	3760	3741	19
2.	11 minutes	3900	3869	31
3.	10½	3624	3620	4
4.	10½	3570	3550	20
5.	11	3685	3653	32
6.	11	3380	3355	25
7.	10½	3180	3141	39
8.	10½	3360	3298	62
9.	10½	3290	3267	23
10.	11	3580	3543	37

In this last experiment we ascertained, that the expired gas contained 8 per cent of carbonic acid.

·08 of carbonic acid in the last.

The breathing in these cases was as nearly natural as we conceive it possible to be in any apparatus: the operator was scarcely fatigued, and his pulse not raised more than about one beat in a minute; the respirations however were deeper and fewer than natural, amounting only to about 58 in eleven minutes, whereas from repeated observations at different and distant times he makes 19 in a minute. The smallness of the deficiency surprised us very much, as, from the reports of other experimenters, we had been prepared to expect a much greater loss. It might be objected, that the air was rarefied by passing through the lungs; but this was almost immediately counteracted by the mass of quicksilver in the gasometers, which amounted at least to one hundred and fifty pounds; and we have repeatedly noticed, that air under these circumstances has suffered no perceptible diminution by standing for a considerable time; in one case, in which air from the lungs was driven into the mercurial gasometers for twenty-seven minutes, the temperature of the quicksilver at the end of the experiment was not raised half a degree of Farenheit's thermometer. The deficiency, in our opinion, principally arises from the difficulty in bringing the lungs precisely to the same state after, as before the experiment; and it must be recollected, that the operator commenced by a forcible expiration into the open air, but finished by a forcible expiration into the mercurial gasometer.

The breathing nearly natural but rather slower.

A greater deficiency expected;

but even this arises principally from air left in the lungs.

Now,

Now, although this gasometer was counterpoised by weights in the scale attached to it, yet we can easily conceive, that more resistance might be afforded to the complete evacuation in the latter case than in the former, and consequently the lungs might contain a few inches more after the experiment than before it, which might in some measure account for the deficiency.

11th experiment.

In the eleventh experiment, portions of gas were taken off from each of the mercurial gasometers as they were filled, and these portions being afterward mixed were carefully examined.

Eleventh Experiment.

Barom.	Thermom. Faht.	Time.	Cubic inches of common air inspired.	Cub. in. expired.	Deficiency.
30·4	50°	11 min.	3460	3437	23

All the experiments made with great care.

To prevent repetition, we shall here state, that all the trials were made in the same manner, and with the same apparatus, namely, the eudiometer, described in the Society's Transactions for 1807, in which one cubic inch is divided into one hundred parts*; and that in almost every instance we made two, and sometimes three experiments on the same gas, and derived fresh confidence from the remarkable coincidence and uniformity of the results. No precaution was at any time omitted which appeared to us necessary to insure accuracy.

Component parts of the expired gas,

One hundred parts of the expired gas being agitated with limewater in the eudiometer, the limewater became turbid, and 8·5 parts of the gas were absorbed, which were consequently carbonic acid; the remaining 91·5 parts were treated with the green sulphate of iron, saturated with nitrous gas, as recommended by professor Davy, and afterward with the simple solution of the green sulphate, when 12·5 parts were absorbed, which were consequently oxigen, and the remaining 79 azote.

* See our Journal, vol. XIX, p. 86.

100 parts of the expired gas therefore consisted of:

8·5	carbonic acid.
12·5	oxigen
79·	azote.
100.	

The air contained in the water gasometer, previous to the experiment, being examined by the same tests, consisted in 100 parts of

and of the air previous to respiration.

21	oxigen
79	azote.
100.	

In trying common atmospheric air with limewater, we could never find any quantity of carbonic acid perceptible in the eudiometer of 100 parts.

No carbonic acid perceptible in common air.

Calculation for Carbonic Acid.

26·55 cubic inches of carb. acid gas given off every minute.

$$100 : 8{\cdot}5 :: 3437 : 292{\cdot}145.$$

So that 292·14 cubic inches of carbonic acid gas were given off in eleven minutes, or 26·55 cubic inches per minute, which is almost exactly the estimate of professor Davy.

In this experiment the operator inspired 3460 cubic inches in eleven minutes, and felt himself in a natural state when he left off. Then, as he makes usually under common circumstances nineteen respirations in a minute.

$16\frac{1}{2}$ cubic inches taken in at an easy inspiration.

$$11 \times 19 = 209 \quad \tfrac{3460}{209} = 16{\cdot}5$$

It follows, that he takes in $16\frac{1}{2}$ cubic inches at every easy inspiration.

As all the experiments had been hitherto made upon the lungs of one person, we concluded, that the next should be performed upon our assistant.

Twelfth Experiment.

12th experiment.

Barom.	Therm. Faht.	Time.	Cub. inches of com. air inspired.	Cub. inches expired.	Difference.
30·3	56°	$5\frac{1}{2}$ min.	3300	3311	11 increase

Here,

Here, as usual, the lungs were exhausted both before, and at the close of the experiment.

An excess of expired air.

The excess of eleven cubic inches, in this case, no doubt, arose from the person not having been in the habit of exhausting his lungs, so that they contained more when he began than when he left off; his lungs appeared to be of greater capacity than those of the usual operator.

Portions of gas were saved from each of the mercurial gasometers as they were filled, which being mixed together, for the average gave the following results:

Component parts of the expired gas.

100 parts of the mixture contained

8·5	carbonic acid
12·5	oxigen
79	azote.
100.	

Calculation for Carbonic Acid.

100 : 8·5 :: 3311 : 281·43.

Consequently 281·43 cubic inches of carbonic acid gas were given off in $5\frac{1}{2}$ minutes.

The carbonic gas is in proportion to the air respired.

In this experiment we meet with a remarkable fact, viz. that as much carbonic acid gas was given off in $5\frac{1}{2}$ minutes, as in the former experiment in eleven minutes; so that it appears, whenever atmospheric air is taken into the lungs, it returns charged with about 8 per cent of carbonic acid. The faster respiration is performed, the more carbonic acid is given off, and consequently the more oxigen consumed: in this instance it was given off at the rate of fifty-one cubic inches per minute.

Thirteenth Experiment.

13th experiment with three times the quantity of air.

We now proceeded to carry on the respiration of common air for a much longer period than usual, and of course on a much larger quantity. The experiment was made by the same operator who had performed all the others, except the 12th. Eleven mercurial gasometers having been filled, taken off, and registered, the operator continued to breathe in the 12th until a mark was made by his colleague upon the

the scale of the water gasometer, and it was again filled with common air to the usual division on the scale. This occupied but a very short space of time. The operator, without taking his lips from the tube, then filled twelve more of the mercurial gasometers, which were registered as before, and he continued to breathe in the 12th, until the water gasometer was again replenished; eleven more were then filled, and portions saved from each: the experiment was completed by a forcible expiration of 166 cubic inches into the 12th; and this last portion being left for an hour and a half was not perceptibly diminished in volume.

Barom.	Therm. Faht.	Time.	Cub. inches of common air inspired.	Cub. inch. expired.	Defic.
29.85	68°	24′ 37″	9890	9872	18

The breathing was so nearly natural that the operator was scarcely fatigued, and thought that he could have gone on for a much longer time.

Increase or decrease of air merely accidental.

The smallness of the deficiency, notwithstanding the experiment occupied 24½ minutes, is a striking circumstance, and leads us to suspect still more strongly, that the deficiency principally arises from the impossibility of always bringing the lungs to the same state after forcible expiration.

Component parts of the expired gas.

100 parts of the mixture of expired gas gave
8 carbonic acid
13 oxigen,
79 azote,

100

Calculation for Carbonic Acid.

100 : 8 : : 9872 : 789·76.

Carbonic acid.

So that 789·76 cubic inches of carbonic acid gas were given off in 24½ minutes, which gives thirty-two cubic inches per minute. But here it must be noticed that the respiration was more rapid than in the 11th experiment, and a larger quantity of carbonic acid given off in the same time. This agrees with the 12th experiment.

We

In ordinary respiration much air returned unaltered.

We are very much inclined to think, that in ordinary respiration, a great part of the air is returned unaltered, viz. that contained in the fauces, in the trachea, and probably a portion of that in the larger branches of the bronchia. If this circumstance be not adverted to in experiments upon small quantities of air, the results can never be correct. There is even a considerable difference in the quality of the first and last portions of a single inspiration. In some experiments made with a view to this subject, a small quantity of the first portions, given off in a common and natural expiration, was received in a vessel over mercury; on examination, it contained only 3·5 per cent carbonic acid; in other experiments the first portions contain from three to five per cent; while the general average appears by the 11th, 12th, and 13th experiments, to be about eight.

204 cubic inches expired contained ·095 of carbonic acid.

The operator, after rather more than a natural inspiration, expired 204 cubic inches into the mercurial gasometer, making his utmost efforts to press as much as possible out of the lungs. These contained 9·5 per cent of carbonic acid, Here we are to recollect, that these 204 cubic inches contained the first, as well as the last portions; the first portions have been proved to contain only from three to five per cent; consequently the last portions must contain more than appears by the average; that is, more than 9·5 per cent.

It now appeared to us of consequence to ascertain exactly what happened to a given volume of atmospheric air, when it was inspired and expired as often as possible.

Fourteenth Experiment.*

14th experiment. Air repeatedly respired.

Three hundred cubic inches of atmospheric air were admitted into the mercurial gasometer G 1; the other, G 2, was empty. The nose being properly secured, and the mouth applied to the tube A, as usual, air was drawn from G 1, and by half turning the cock 2, was expired into G 2. This was repeated until the contents of G 1 had been made to pass through the lungs, and transmitted to G 2. The air

* In this experiment there was obviously no occasion to make allowance for the air contained in the tubes and sockets. We find its volume to be eighteen cubic inches.

air was then inspired from G 2, and expired into G 1, until G 2 was nearly empty. This was repeated about eight or ten times during three minutes, until respiration became extremely laborious, and the operator desisted.

The whole 300 cubic inches must have passed eight or ten times through the lungs; and we confidently expected, that on examining the air we should have found an unusual proportion of carbonic acid.

Component parts of the respired air.

But 100 parts gave only 9·5 carbonic acid,
5·5 oxigen,
85· azote,

100

Here was an increase of six parts in 100 of something which the tests for oxigen would not take up, and also a loss of six per cent oxigen. This seemed to convince us, that under certain circumstances, as during some peculiar alteration in the vital functions, gaseous oxide of carbon, carburetted hidrogen, or some other gas not absorbable by lime water or the tests for oxigen, might be given off from the lungs, and we accordingly determined to repeat Cruikshank's experiments with hyperoxigenized muriatic acid gas.

Gaseous oxide of carbon converted into carbonic acid by hyperoxigenized muriatic acid gas.

We procured the gas from hyperoxigenized muriate of potash by means of muriatic acid, and mixing it with a known portion of gaseous oxide of carbon in a flint stopper bottle, the mouth of which was immersed in mercury for twenty-four hours, the gaseous oxide of carbon was converted into carbonic acid gas, as was proved by its effects upon lime water, which, when both the gasses are pure, absorbs them entirely after they have remained together for twenty-four hours; it was plain, therefore, that we had the means of detecting gaseous oxide of carbon, and doubtless carburetted hidrogen, if any should be contained in the expired gas. From a conviction of the importance of these experiments we were determined to take nothing upon trust.

Fifteenth Experiment.

15th experiment.

We repeated the 14th experiment with a little variation. In this case we employed only one of the mercurial gasometers,

Air repeatedly respired.

ters, into which exactly 300 cubic inches of atmospheric air were admitted. The operator, having made an easy expiration, applied his mouth to the cock at the top of the bell glass, and the time being noted, began to breathe; in less than a minute he found himself obliged to take deeper and deeper inspirations; and at last the efforts of the lungs to take in air became so strong and sudden, that the glass was in some danger of being broken against the side of the gasometer. A great sense of oppression and suffocation was now felt in the chest, vision became indistinct, and after the second minute his whole attention seemed to be withdrawn from surrounding objects and fixed upon the experiment. He now experienced that buzz in the ears which is noticed in breathing nitrous oxide, and after the third minute had only sufficient recollection to close the cock after an expiration. This secured the result of the experiment; but he became so perfectly insensible, that, on recovering, he was much surprised at finding his friend and the assistant on the table in the act of supporting him. It was noticed that he made thirty-five inspirations during the experiment. We now examined the air which had been so treated.

The air examined.

100 parts contained 10 carbonic acid,
4 oxigen,
86 azote,

100

In this experiment it is remarkable, that the air which had been so often through the lungs should only have furnished 10 per cent of carbonic acid, while the air which passes them but once contains from 8 to 8·5.

Here the oxigen had lost 7 from 21, and the azote had gained 7 upon 79.

We knew by previous experiment*, that every cubic inch of carbonic acid gas required exactly a cubic inch of oxigen for its formation; the ten parts of carbonic acid may therefore be reckoned as oxigen, which would make the constitution of

* See the experiments on carbonic acid in the Society's Transactions, or our Journal, vol. XIX, p. 225.

the

the gas after the experiment $\begin{cases} 14 \text{ oxigen,} \\ 86 \text{ azote,} \end{cases}$

whereas before the experiment it was $\begin{cases} 21 \text{ oxigen,} \\ 79 \text{ azote.} \end{cases}$

Now we did not suppose the residuum of 86 to be all azote, though 79 might be; therefore seven parts appeared to have been added by this unnatural mode of respiring, and we conjectured the addition might be gaseous oxide of carbon. Residuum examined.

To ascertain this, we put 40 parts into a flint stopper bottle, and nearly filled it with about 100 parts hyperoxigenized muriatic acid, procured as before, and recently prepared; the stopper being put in, over distilled water, we plunged it into quicksilver, and filled a second bottle in the same way, as a comparative experiment.

We next procured some pure azote, by absorbing the oxigen from a portion of atmospheric air by the saturated green sulphate and simple green sulphate as usual; 40 parts of this azote were mixed with the same proportion of the acid gas as in the other experiment, and the whole suffered to stand for forty-eight hours; at the end of this time the azote was examined, by washing it first in distilled water, and afterward in the eudiometer with the tests for oxigen; and there were still exactly 40 parts left; proving that the hyperoxigenized muriatic acid gas has no action upon azote. Hyperoxigenized muriatic acid gas has no action upon azote.

We then examined the bottles containing the residuum from the air that had been so often respired, and found that it had not experienced the slightest change; it was therefore plainly azote; and on reflection, it occurred to us, that if a certain proportion of oxigen had been absorbed or lost in any way, while the azote remained unaltered, there must be an increased proportion of the latter. The residuum azote.

Now we knew exactly both the bulk and the constitution of the air before the experiment; but it was impossible to know the bulk or volume after the experiment otherwise than by calculation.

The 300 cubic inches of atmospheric air before the experiment

riment contained 21 oxigen and 79 azote in 100 parts, making the total quantity of oxigen 63 cubic inches,

azote 237

300

The lungs absorb little if any azote.

Now if the lungs be capable of fixing permanently any azote from the atmosphere, it appears by our experiments, that the quantity must be very minute, seeing that in the 11th, 12th, and 13th experiments, it did not disturb the proportion of azote, as shown by the eudiometer; we shall therefore in the present instance assume the volume of azote after the experiment at 237 cubic inches, as before.

But after the experiment, every 100 parts consisted of 86 parts azote, and 14 oxigen, either in the form of carbonic acid, or free.

$$86 : 14 :: 237 : 38{\cdot}58.$$

Therefore the total quantity of oxigen left after the experiment would have been 38·58 cubic inches.

Then 237 azote + 38·58 oxigen = 275·58;

the quantity of gas after respiration would therefore have been 275·58 cubic inches.

300 — 275·58 = the loss of oxigen, or 24·42 cubic inches.

Oxigen absorbed during respiration.

It appears, therefore, that 24·42 cubic inches of oxigen had been absorbed by the system under the circumstances of this experiment.

Reviewing the 14th experiment, it appears, that the gas after respiration contained 85 per cent azote, and 15 per cent oxigen, either in the state of carbonic acid, or free.

State of the air in exp. 14.

State of the Air before the Experiment.

300 = 237 azote + 63 oxigen.

After the Experiment.

$$85 : 15 :: 237 : 41{\cdot}82.$$

The total quantity of oxigen after the experiment appears to be 41·82 cubic inches.

Then 237 azote + 41·82 oxigen = 278·82.

The

The total volume after the experiment appears to be 278·82 cubic inches.

$$300 - 278{\cdot}82 = 21{\cdot}18.$$

The loss of oxigen in this case was 21·18 cubic inches. Oxigen absorbed.

We are disposed to consider the 11th as a standard experiment relative to carbonic acid gas, because the quantity of air respired in a given time is pretty near the average of the first ten experiments; and because it very nearly agrees with the statement of Professor Davy. In this experiment 292 cubic inches of carbonic acid gas were given off in eleven minutes; the barometer was 30·4, the thermometer 50°, the volume being calculated at the mean, viz. barometer 30, thermometer 60°, will be 302 cubic inches given off in eleven minutes, or 39534 cubic inches in twenty-four hours, supposing the production to be uniform during all that period; and as 100 cubic inches of carbonic acid gas weigh 47·26 grains,

According to experiments, above 11 oz. troy of carbon emitted from the lungs daily, and 39534 cub. inches of oxigen gas consumed

$$100 : 47{\cdot}26 :: 39534 : 18683{\cdot}76;$$

the weight of the carbonic acid gas amounts to 18683.76 grains; and estimating the carbon in it at 28 parts in 100, according to Lavoisier, or 28·60, as calculated in the experiments on diamond, recorded in the Society's Transactions,

$$100 : 28{\cdot}60 :: 18683{\cdot}76 : 5363{\cdot}55 \text{ grains};$$

it will follow, that 5363·55 grs. or above 11 oz. troy of solid carbon, are emitted by the lungs in the course of twenty-four hours; and that 39534 cubic inches of oxigen gas are consumed in the same time. But when we consider, that in respiration perfectly natural a much smaller quantity of air can come in contact with those parts of the lungs calculated to act upon it, the proportion of carbonic acid gas given off in natural respiration ought probably to stand considerably lower than in the above estimate; but at all events it will be very considerable.

But this more than the common average.

Sixteenth Experiment.

Having made so many experiments upon atmospheric air, we now proceeded to ascertain the effects produced upon

16th experiment.

Oxigen gas respired.

upon oxigen gas by the process of respiration. The water gasometer was filled with oxigen gas made from the hyperoxigenised muriate of potash by heat, care having been taken to clear all the tubes, &c. as much as possible of common air, by forcing a quantity of oxigen gas through them.

One hundred parts from the water gasometer being treated with the usual tests in the eudiometer, a residuum of only 2·5 was left; so that 97·5 per cent were pure oxigen, and the rest azote.

Pulse quickened,

The register of the water apparatus being noticed, and the operator having prepared himself as usual by a forced expiration, began to respire; his pulse was 72; and at the end of 9 minutes and twenty seconds, the experiment was concluded by a forced expiration, when the pulse was raised to 88.

Barom.	Therm. Faht.	Time.	Cubic Inches of oxigen gas inspired.	Cub. Inches expired.	Deficiency.
29·5	53°	9′20″	3260	3193	67

and natural heat increased.

The operator felt a general glow over the body to the very extremities, with a gentle perspiration; this however went off in a few minutes, and no remarkable deviation from the ordinary state was experienced.

Component parts of the expired gas.

A portion having been saved, as usual, from each of the mercurial gasometers, for an average,

100 parts contained

11 carbonic acid,
83 oxigen,
6 azote,

100

The examination repeated gave the same results.

Calculation for Carbonic Acid.

100 : 11 : : 3193 : 351·23,

consequently, 351·23 cubic inches of carbonic acid gas were formed in 9′20″, or 37·64 cubic inches in a minute.

More carbonic acid formed.

Here it is plain, that a greater quantity of carbonic acid was formed from oxigen than from common air, in the same time,

time, and hence we infer, that one use of azote is to regulate the quantity of oxigen, which shall be taken up in the act of respiration. Use of azote in the air.

The gas inspired was 3260 cubic inches, and of this 2·5 per cent were azote.

$$100 : 2{\cdot}5 :: 3260 : 81{\cdot}50.$$

The total quantity of azote in the gas inspired was therefore 81·50 cubic inches.

The quantity of gas expired was 3193 cubic inches, and of this every 100 parts contained six of azote.

$$100 : 6 :: 3193 : 191{\cdot}58.$$

The total quantity of azote in the gas expired was therefore 191·58 cubic inches; but the total quantity of azote before respiration was only 81·50.

$$191{\cdot}58 - 81{\cdot}50 = 110{\cdot}08;$$

The azote apparently increased during respiration.

therefore 110·08 cubic inches were added by the process of respiration, beside what little remained in the lungs after the experiment.

Calculation for Oxigen.

The 3260 cubic inches of gas inspired contained 81·50 azote. Calculation of the oxigen.

$$3260 - 81{\cdot}50 = 3178{\cdot}50,$$

and consequently the pure oxigen was 3178·50 cubic inches. The 3193 cubic inches of gas expired contained 83 per cent of free oxigen, and 11 per cent in carbonic acid gas, making together 94.

$$100 : 94 :: 3193 : 3001{\cdot}42.$$

The oxigen gas, found after the experiment, was therefore 3001·42 cubic inches, and deducting this from the oxigen before the experiment,

$$3178{\cdot}50 - 3001{\cdot}42 = 177{\cdot}08.$$

It appears, at first sight, that 177·08 cubic inches of oxigen were missing, but great part of this may be accounted for, by adverting to the state of the lungs after the experiment. Portion missing.

The addition to the azote from the air in the lungs.

The addition of 110·08 cubic inches of azote we consider as arising from that portion still retained in the lungs, notwithstanding the forced expiration at the beginning of the experiment: and con dering that in the 14th and 15th experiment, where the same air was repeatedly breathed, the proportion of azote was in the one case 85, and in the other 86 per cent; it seems fair to presume, that the residual air contained in the lungs after a forced expiration may amount in 100 parts to not more than 16 oxigen and 84 azote. Any one who reflects upon the structure of the lungs, and the minute ramifications of the extremities of the bronchial vessels; when he also considers, that those parts of the lungs with which the air comes into contact, if spread out, would present a surface equal to that of the superficies of the whole body; and lastly, that this viscus is so exceedingly spongy and porous, that when once inflated, it is ever after impossible by ordinary mechanical means to expel the air completely; he will easily perceive, not only that a large portion of air must remain for a considerable time in contact with the internal surface of the lungs, where it is liable to lose a portion of its oxigen, but also that the residual quantity of air in the lungs, after the most violent attempts at expiration, may be very considerable. It is to this circumstance, that we attribute the excess of azote in the experiments upon oxigen, and pretty deep inspirations of this gas having been made during 9′20″, the azote must have been in great measure displaced. Admitting then, that the air contained in the lungs, before the experiment, consisted of 16 oxigen, 84 azote, and at the conclusion of the experiment of 94 oxigen, 6 azote, we have

$$\frac{84\,x}{100} \text{ azote at the beginning,}$$

$$\frac{6\,x}{100} \text{ azote at the end.}$$

$$\frac{6\,x}{100} + 100 = \frac{84\,x}{100},$$

$$110 = \frac{84\,x}{100} - \frac{6\,x}{100} \text{ or, } 84\,x - {\cdot}06\,x = {\cdot}78\,x.$$

$$x = \frac{110}{{\cdot}78} \text{ or 141 cubic inches;}$$

Therefore

Therefore upon this calculation it appears, that 141 cubic inches of gas remained in the lungs after a forcible attempt at expiration; then the state of the lungs before the experiment must have been

State of the lungs before the experiment,

118·44 azote,
22·56 oxigen.

141

And after the experiment,

and after.

132·54 oxigen,
8·46 azote.

141

Calculation on total Quantities.

Calculation on the whole quantities.

Azote before the experiment	81·50 cubic inches,
—— contained in the lungs	118·44
	199·94
Azote after the experiment,	
—— found by the tests,	191·58
—— contained in the lungs,	8·46
	200·04
Oxigen before the experiment,	3178·50
—— contained in the lungs,	22·56
	3201·06
Oxigen after the experiment,	
Found by the tests,	3001·42
Contained in the lungs,	132·54
	3133·96
Total of oxigen before the experiment,	3201·06
Total of oxigen after the experiment,	3133·96
Difference	67·10

The deficiency noticed in the experiment was 67, supposing that the lungs were brought to the same state after as before the

Deficiency of oxigen.

the experiment; but granting that this was not the case, and that at the close of the experiment the state of the lungs was 141 + 67 = 208, still our approximation will come within four or five cubic inches, for the azote contained in the sixty-seven missing would be only about four cubic inches. We are aware, that, the temperature of the lungs being 97, while that of the gas was 53°, the 141 cubic inches would occupy a space equal to 154 cubic inches; but this residual quantity must be greater or less according to the exertion made, and also probably according to the state of the muscular fibre at the time.

Seventeenth Experiment.

17th experiment.

The water gasometer was filled to the usual mark upon the scale with oxigen gas prepared from about 9 oz. troy of hyperoxigenised muriate of potash, as in the former experiment; the gas being examined was found to contain as before, 2·5 azote, and 97·5 oxigen, in 100 parts.

Effects on the operator.

The apparatus being found air tight, and all the tubes &c. cleared of atmospheric air by passing oxigen through them, the operator prepared himself for the experiment; but it must be noticed, that he had been rather fatigued during five hours previous to respiring, and had not taken any refreshment; the weather was very warm; his pulse 86; heat under the tongue 98¾; he felt no uncomfortable sensation during the process, but experienced a gentle glow and universal perspiration, breathing all the time with great ease, his pulse after the experiment 102, and the heat under the tongue 99°.

Results.

Barom.	Therm. Faht.	Time.	Cub. inches of oxigen gas inspired.	Cub. inches expired.	Deficiency.
30·3	70°	7′25″	3420	3362	58.

The quantities of expired gas taken off in each of the mercurial gasometers were as under, in the order in which they were filled.

No.

No. 1....250 cubic inches.	No. 8....296 cubic inches.
2....290	9....256
3....272	10....256
4....238	11....286
5....252	12....257
6....300	13....168
7....241	
	3362

The 13th gasometer was the whole of the last single and forcible expiration, portions were saved from each of the gasometers, and we first examined the state of No. 1.

Examination of the expired air.

100 parts contained	9 carbonic acid,
	25 azote,
	66 oxigen,
	100

The large quantity of azote in this case was a clear proof, that our conjecture upon the residual gas in the lungs was well founded.

We then examined a mixture of No. 2 and 3.

100 parts contained	10·5 carbonic acid,
	10 azote,
	79·5 oxigen,
	100

Here the quantity of azote was diminishing, and the ratio of carbonic acid increasing, so that it appears necessary for the lungs to be cleared of azote, before the increased proportion of carbonic acid can take place.

The 13th or last gasometer was now examined by itself;

100 parts contained	12·5 carbonic acid,
	5·5 azote,
	82 oxigen.
	100

Here the proportion of azote was only 3 per cent more than what existed previously in the gas, and hence we may conclude, that even seven minutes and a half were not sufficient

cient time to remove the azote from the extremities of the bronchia.

We lastly made a mixture of all the gasometers, from 2 to 12 inclusive, and found that 100 parts contained

12	carbonic acid,
6·5	azote,
81·5	oxigen,
100.	

Calculation for Carbonic Acid.

Carbonic acid calculated.

100 : 9 : : 250 : 22·50 Carbonic acid gas in No. 1. 22·50
100 : 12·5 : : 186 : 21 ditto No. 13. 21

From 3362 total expired

250 No. 1.
168 No. 13.

Deduct 418

Leaves the mixture 2944 of No. 2 to No. 12.

100 : 12 : : 2944 : 353·28 Carb. acid gas in mixture 2 to 12. 353·28

396·78

The total quantity of carbonic acid gas emitted was therefore 396·78 cubic inches.

Calculation for Azote.

Azote calculated.

100 : 25 : : 250 : 62·50	Azote in No. 1.	62·50
100 : 5·5 : : 168 : 9·24	—— in No. 13.	9·24
100 : 6·5 : : 2944 : 191·36	—— in mixt. 2 to 12.	191·36
		263·10

The azote expired, beside what might be contained in the lungs at the close of the experiment, was therefore 263·10 cubic inches. Here it is plain, that the operator, at the beginning of this experiment, had not brought his lungs to the same state as in the preceding; or that in consequence of fatigue, and want of refreshment for several hours,

hours, the proportion of azote in the lungs might be greater.

Every 100 parts of oxigen, before it was inspired, contained 2·5 azote.

$$100 : 2{\cdot}5 :: 3420 : 85{\cdot}50;$$

Consequently it contained 85·50 cubic inches of azote.

From 263·10
Deduct 85·50 the original azote,

177·60 will be left for the increase of azote.

Then supposing as before, that the quality of the air in the lungs before the experiment was 84 per cent azote, 16 oxigen; and after the experiment 5·5 per cent azote, 94·5 oxigen, as found in the last gasometer, we take

$\frac{84\,x}{100}$ azote at the beginning,

$\frac{5{\cdot}5\,x}{100}$ azote at the end;

$$\frac{5{\cdot}5\,x}{100} + 177{\cdot}60 = \frac{84\,x}{100}.$$

$$177{\cdot}60 = \frac{84\,x}{100} - \frac{5{\cdot}5\,x}{100} \text{ or } {\cdot}84x - {\cdot}055\,x = {\cdot}785\,x,$$

$$x = \frac{177{\cdot}60}{{\cdot}785} \text{ or } 226 \text{ cubic inches.}$$

Hence it appears, that previous to the experiment the lungs contained in this instance 226 cubic inches; and if we suppose them to be in the same state after as before the experiment, the quality of the gas in each case will be as follows:

Contents of the Lungs before the Experiment.

Contents of the lungs before

189·84 cubic inches of azote,
36·16 oxigen,

226

Contents

Contents of the Lungs after the Experiment.

and after the experiment.

12·43 cubic inches of azote,
213·57 oxigen,

226

Calculation for Oxigen.

Oxigen calculated.

3420 — 85·50 = 3334·50 original oxigen,
Add 36·16 in the lungs before the experiment,

3370·66 { total quantity of oxigen before the experiment.

After the Experiment.

100 : 66 :: 250 : 165	oxigen in No. 1.	165
100 : 82 :: 168 : 137·76	—— in No. 13.	137·76
100 : 81·5 :: 2944 : 2399·36	—— in mixt. 2 to 12.	2399·36
	—— in carbonic acid	396·78
	—— in lungs after expt.	213·57
		3312·47

3370·66 original oxigen,
3312·47 after experiment,

58·19 deficiency.

Deficiency.

The observed deficiency in this experiment was 58.

The deficiency in this case, and in the former experiment with oxigen, though comparatively small, when contrasted with the quantity of gas respired, is larger than the average with atmospheric air; it seems probable, therefore, that a portion may be detained in the system. It must be remembered, that what we call residual gas is not only that contained in the substance of the lungs, and in its appendages, but also that contained in the fauces and mouth.

Eighteenth Experiment.

18th experiment.

Barom.	Therm.	Time.	Cub. inches of oxigen gas inspired.	Cub. inches expired.	Deficiency.
30·15	70°	8′45″	3130	3060	70.

The operator breathed as usual, after having made a strong effort to exhaust his lungs; his pulse before the experiment

periment was 84, the thermometer under his tongue 98°: after the experiment his pulse was 96, and the thermometer under his tongue still 98°; the same gentle glow and perspiration was felt as in the other experiments on oxigen; a portion of the gas was saved from each of the mercurial gasometers, and their amounts were as under: Effects.

Results.

1.	196	7.	258
2.	228	8.	272
3.	284	9.	250
4.	294	10.	304
5.	248	11.	223
6.	280	12.	223
			3060

No. 1, tried by itself, contained in 100 parts, State of the expired air.

9 carbonic acid,
22 azote,
69 oxigen,

100

No. 12, the last, contained in 100 parts,

12 carbonic acid,
5 azote,
83 oxigen,

100

On account of an accident we cannot give the proportions contained in 2 to 10; but the contents of the first and last gasometers confirm the former experiments, and shows that the proportion of azote continues to diminish, as the experiment proceeds; and also that there is a larger proportion of carbonic acid given off when oxigen is employed, instead of atmospheric air.

In this recital of experiments, which have occupied a considerable portion of time and attention, we have endeavoured to give a plain statement of facts, from which every one may draw conclusions for himself; we shall here, however, take the liberty of briefly recapitulating the principal of those Leading facts.

facts,

facts, and submitting what seems to us the most obvious inferences.

No water formed in the lungs.

1. It appears that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxigen consumed, and therefore there is no reason to conjecture, that any water is formed by a union of oxigen and hidrogen in the lungs.

Carbonic acid formed.

2. Atmospheric air once entering the lungs returns charged with from 8 to 8·5 per cent of carbonic acid gas; and when the contacts are repeated almost as frequently as possible, only 10 per cent are emitted.

The 12th and 13th experiments prove, that, when the inspirations and expirations are more rapid than usual, a larger quantity of carbonic acid is emitted in a given time, but the proportion is nearly the same, or about 8 per cent. The proportions of carbonic acid gas, in the first and last portions of a deep inspiration, differ as widely as from 3·5 to 9·5 per cent.

Average proportions.

3. Considering the 11th as a standard experiment, it appears, that a middle sized man, aged about thirty-eight years, and whose pulse is seventy on an average, gives off 302 cubic inches of carbonic acid gas from his lungs in eleven minutes; and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains; the carbon in which is 5363 grains, or rather more than 11 oz. troy. The oxigen consumed in the same time will be equal in volume to the carbonic acid gas; but it is evident, that the quantity of carbonic acid gas, emitted in a given time, must depend very much upon the circumstances under which respiration is performed; and here it may be proper to notice, that all the experiments were made between breakfast and dinner.

Under some circumstances oxigen absorbed.

4. When respiration is attended with distressing circumstances, as in the 14th and 15th experiments, there is reason to conclude, that a portion of oxigen is absorbed; and in the last of these experiments we may remark, that, as the oxigen decreases in quantity, perception gradually ceases, and we may suppose, that life would be completely extinguished on the total abstraction of oxigen.

5. A

5. A larger proportion of carbonic acid gas is formed by the human subject from oxigen, than from atmospheric air.

Oxigen gas alone forms most carbonic acid.

6. An easy, natural inspiration is from 16 to 17 cubic inches in the subject of these experiments, who makes about 19 in a minute; this, however, will vary in different individuals; and perhaps we ought to estimate the quantity of carbonic acid gas, given off in perfectly natural respiration, at somewhat less, and most likely at considerably less, than in the statement above, when we consider, that in short inspirations the quantity of air which has reached no farther than the fauces, trachea, &c., bears a much larger proportion to the whole mass respired, than when the inspirations are deep.

7. No hidrogen, or any other gas, appears to be evolved during the process of respiration.

No other gas evolved in respiration.

8. The general average of the deficiency in the total amount of common air inspired, appears to be very small, amounting only to about 6 parts in 1000, and we are inclined to attribute it in great measure to the difficulty in exhausting the lungs as completely after an experiment as before it; the first expiration being made into the open air, the last into the apparatus.

Deficiency of the air very small.

9. The experiments upon oxigen gas prove, that the quantity of air remaining in the lungs and its appendages is very considerable, and that, without a reference to this circumstance, all experiments upon small quantities of gas are liable to inaccuracy.

Quantity retained in the lungs considerable.

Other important conclusions might perhaps be drawn from the facts related in this paper, but having already trespassed largely upon the time of the Society, we shall abstain from any farther remarks, until we bring forward a new series of experiments.

V.

V.

A Letter on Comets, addressed to Mr. Bode, *Astronomer Royal at Berlin. Received from the Author.*

SIR,

Occasion of the letter.

YOU wished to have in writing the conjectures on the nature of comets, which I had the honour of mentioning to you in conversation a few days ago. At present however they form but the embryo of a system, fully to unfold which would require studies I have not pursued, particularly that of the astronomical history of these bodies, and of the opinions that have been entertained respecting them. I cannot refuse, however, to deliver into your hands these rudiments of ideas, relying on your indulgence both with respect to their want of precision, and to the brevity with which I treat the physical principles, on which they are founded; as these principles are more fully exhibited in different works of mine. But I will at least attempt to explain in a general way the consequences I deduce from these principles with respect to the *luminous* appearances of comets; persuaded, that, if they contain a single seed of truth, your intimate acquaintance with the heavenly bodies will enable you to discern and expand it.

Its subject.

All luminous substances give out light in consequence of chemical decomposition.

I shall set out with what the whole of our knowledge of nature appears to me to teach us very clearly respecting the substances, that are capable of becoming *luminous*: this is, that the *light*, which then escapes from them, had entered into their *composition* as an ingredient, and is evolved by chemical *decomposition*.

Light of the Sun from a fluid surrounding it.

In applying this principle to the heavenly bodies, observation has added one circumstance with respect to the Sun, which may be extended by analogy to the *fixed stars*. Dr. Herschel, you know, has discovered, that the *light*, which issues from the *Sun*, does not come directly from its *solid substance*, but from an atmosphere, or some fluid, by which it is surrounded. Thus this grand phenomenon has been brought nearer to us as it were; analogy, though the sub-

Luminous phenomena of

jects

jects differ extremely in magnitude, connecting it with the *luminous* phenomena observed in our atmosphere, such as the *aurora borealis*, *luminous* clouds, and many other luminous meteors. All these are so many *luciferous* vapours, though of different kinds; which, being raised in the atmosphere in their *compound* state, and meeting with something to *decompose* them, emit the *light*, that entered into their composition.

our atmosphere.

To determine with more precision what relates to the Sun, we should consider what led Dr. Herschel to the discovery, that its *light* did not issue directly from its *solid* body, but from an atmosphere surrounding it. This was its changeable *spots*; a phenomenon, that had greatly perplexed astronomers and natural philosophers, but which is clearly explained by the discovery of Dr. Herschel, that these spots are the *body* of the Sun itself, *nonluminous*, and seen through transparent parts of its *luminous* envelope. This shows, that, though the *light* issues from a fluid belonging to that kind of atmosphere, which surrounds the Sun, this fluid is not the atmosphere itself, but is simply mixed with it; and this not completely throughout, since there are parts that remain *transparent*, or from which no *light* emanates.

Proof that the light of the Sun is not from its body.

Another consideration determines this phenomena still more precisely. The *disk* of the Sun, as measured by us, is properly that of its *lucid covering*, in the edge of which Dr. Herschel has discerned *gaps*, when any large *spot* arrives there. This *covering* then has a constantly uniform *thickness*, since we perceive no change in the diameter of the Sun, which is determined by it. Now this is not the character of an *atmosphere*, which in the general acceptation of the term, taken from the atmosphere of the Earth, is considered as formed of one or several *expansible fluids*, the density of which diminishes in proportion as the strata are more remote from their base, so that they extend by imperceptible gradations to an indefinite distance. In fact, when astronomers have spoken of an atmosphere of the Sun, they have ascribed to it a vast extent. Thus, if the *luciferous* fluid, that surrounds the Sun, have such a constant thickness, and be so well defined, that it has hitherto been taken for

But it is from a permanent lucid covering, not of the nature of an atmosphere,

but rather of a stratum of clouds.

for the body of the Sun itself; it must be a kind of *vapour*, which, emanating incessantly from the Sun, always rises to the same height in its *atmosphere*, and there forms a stratum as it were of clouds, which is decomposed at a certain height, like the *luminous clouds* that are sometimes formed in our atmosphere. By this *decomposition* the light escapes; and the substances, with which it was combined, must form some kind of dew, that falls back again upon the Sun. Now the *transparent* spaces, through which we see the opake body of the Sun, are parts of its atmosphere, into which the *luciferous* vapour does not ascend, or where, when it arrives there, it does not find the ingredients necessary to decompose it into *clouds*. The latter case would be analogous to what we see take place in our own atmosphere, when through the intervals of the clouds we discern the blue colour of the transparent air. The matter of clouds is there, since, as I have proved, they are formed by the decomposition of the air itself; but in these spaces the ingredient, which decompose the air, and which assuredly rises from the Earth, is not present.

Spots in the Sun.

Openings between our clouds.

The same theory applicable to other celestial bodies, but with certain modifications.

This theory of the luminous effects of the Sun may be applied directly to the fixed stars; and it may also be extended to other large bodies with some modifications. The atmospheric effects of every one of the bodies dispersed through space depend on its particular *constitution*. To the constitution of the Sun and fixed stars are owing the greatness and constancy of the luminous phenomena, by which they are distinguished. But other bodies, from the difference of their constitutions, may produce phenomena of a similar kind, though much inferior in different degrees, more or less rarely, and under different appearances. Our Earth, though of the lowest order in this respect, maintains its analogy by the *phosphoric* phenomena of its atmosphere; and there may be many intermediate degrees between the highest and this, the lowest.

Comets.

From this last consideration has arisen my conjecture respecting the nature of *comets*; but to establish it demands at least a knowledge of every thing, that has been observed respecting these bodies, which I have not studied. What I have the honour to lay before you therefore, Sir, you

you will consider only as the rudiments of ideas, which observation and reflection may perhaps hereafter confirm.

Their light is from some chemical decomposition,

I have laid it down as a principle, that the *light* which emanates from phosphoric bodies can arise only from a *decomposition*, which liberates this light from its combination with other ingredients, and which must be induced and influenced by preceding chemical actions on these bodies. This general principle is deduced from the experience of all the luminous phenomena, that are exhibited in our chemical processes, and all that we observe to take place spontaneously on our globe: and this is all we can refer to the other large bodies in the decreasing scale of these effects, from the Sun and fixed stars, the light of which is so intense and permanent, to the phosphoric phenomena of our atmosphere. Thus we can determine nothing with respect to the constitution of these bodies, the nature of the chemical processes that extricate light from them, the periods in which these effects must be reproduced, or lastly their different appearances when they are produced. Here then we find many causes of uncertainty in the application of this principle to the phenomena of *comets*; and accordingly every thing I have to say on the subject must be vague and hypothetical.

of the nature of which we know nothing.

Small bodies revolve round the Sun occasionally visible

I conceive, that a great number of bodies revolve round the Sun at different distances, and with different motions; which from the smallness of their size, and more especially from the nature of their surface, cannot be rendered visible to us by reflection of the solar rays; but which at certain times, or under certain circumstances, are capable of producing *luciferous vapours*.

Their luminousness affected by their nearness to the Sun.

Let us suppose, that a certain degree of proximity to the Sun is a circumstance determining either the production or the decomposition of these vapours; in other words, that the intensity of its rays is the chemical cause, that induces the luminous effects. I have been led to this idea by a phenomenon observed in our experiments, which displays some analogy with the supposition here made: certain bodies recently calcined, or calcined anew, such as oyster shells and the Bononian stone, after having been some time exposed to the *rays of the Sun*, are *luminous* in the dark.

Phenomena of the Bolognian phosphorus.

 Euler

Euler imagined, that he could draw from this phenomenon an argument against Newton's theory of the *emission of light*, and in favour of his theory of vibrations: conceiving, that the effect of the rays of the Sun on these bodies was to produce in them *vibrations*, which continued for some time.

Light not a system of vibrations.

On the contrary I deduce from it an argument militating directly against his theory: for he supposes, that the different *colours* of light are different kinds of *vibrations;* so that when these bodies are exposed to rays of certain *colours*, which can produce vibrations of their own kind only, the light afterward emitted by these bodies in the dark must be of the same *colour*. Now from the experiments of Mr. Wilson, which I myself have seen, the light emitted by a calcined oyster shell is always the same, whatever was the colour of the ray, to which it had been exposed. These oyster shells acquire different properties in this respect, according to the degree of calcination, or some other unknown circumstance. I have seen them emit red, green, and yellow light: but they never shine again, each with its own colour, except from the immediate action of the solar rays; and each single coloured ray causes them to emit the same colour, but more faintly, the ray of their *own colour* having no advantage over the rest. Hence I infer, that the rays of the Sun produce a *decomposition* in these calcined substances, in consequence of which they give out the *light*, that entered into their composition. The decomposition is thus accelerated, and goes on for some time in the dark; but without this it would go on too slowly, for the *light* that is evolved to be perceptible; though it does go on, for the calcined oyster shells do not long retain this property.

Comets may differ from each other, and from themselves at different times.

Though a greater proximity of the Sun may thus be the cause of a more rapid decomposition of the luciferous vapours that arise from comets, this approximation cannot produce the same effect either on different comets, or on the same comet on its return to its perihelion, unless the production of these vapours go on constantly with the same degree of intensity; which there is no reason to suppose; for there may be a great difference in this respect between different comets, and in the same comet at different times.

If

If these vapours then rise to a considerable height above a comet, before they are phosphorescently decomposed; not having, like those of the Sun, a fixed limit of ascent; when the comet approaches its perihelion these vapours will become luminous, and the comet may appear as a lucid nebulosity merely, with an irregular boundary, and without our being able to distinguish any determinate disk: and when such a disk is observed, it is probably owing, as in the case of the Sun, to the regularity of the lucid vapour, without the body of the comet, which is surrounded by vapours, being at all visible.

Comets without a nucleus

and with.

If such be the cause of the light emitted by comets; that is, if it be owing to luciferous vapours, which are decomposed; and if the rays of the Sun produce or accelerate this decomposition, when they approach their perihelion; their light may be at first faint, then increase more than in proportion to their approximation, and decrease more rapidly than in proportion to their recession. If then their apparent magnitude enter into the calculation of their orbit, may not this be a cause of errour?

May not their apparent magnitudes lead to errours in calculation.

Farther, as the *light* of comets appears to be certainly *phosphoric*, and not reflected; as *phosphoric* effects unquestionably depend on chemical operations proceeding from the composition of bodies: and as these operations in comets do not appear to follow as regular a course as in the Sun; may not this be the reason, that we are so little capable of recognizing comets on their return, though their elements have been accurately determined, and we have reason to expect their return at stated periods? for they may actually return at these periods without being known, on account of their having a very different appearance; or even without being visible, as the circumstances, which caused them to emit light at the time when they were observed, may not exist. This would render unnecessary the supposition, which some astronomers have adopted, that in their very eccentric paths they meet with bodies, the vicinity of which obliges them to alter their course.

A comet may not be known at its return from the difference of its appearance,

so that a perturbation of its orbit is not necessary to account for it.

One of the most remarkable phenomena of *comets*, the *tail* which they commonly have, excites us strongly to inquire into its cause: but not to give up the reins too much

Tails of comets.

to

to imagination, we must look for some circumstances in the phenomenon itself, and in analogy, to limit our field of inquiry. One circumstance in the phenomenon itself, which tends to answer this purpose, is, that the *tail* of a *comet* is always on the side farthest from the Sun: and analogous to this on our globe we find the *luminous corruscations*, that frequently accompany the *aurora borealis*, likewise take place in our *night*, that is opposite to the Sun. Farther, sometimes we see in the mass of atmospheric fluids, all of which gravitate toward the Earth, a fluid sometimes formed, which, from the prodigious rapidity of its motion in a right line, experiences no perceptible deflection from the action of gravity; which thus, if it dart vertically upward, may soon get out of the sphere of gravitation toward our globe, and which is *luminous* in its course. I mean the *electric fluid*; for this, though little deflected by gravitation in its rapid course, is subjected to another law, which retains it in the atmosphere. This law is its strong tendency to unite with the particles of the *air*, which occasions the *lightning* to disappear in its horizontal movements, after the zigzags it has been forced to make by the resistance of the *air*; zigzags which are not perceptible in the corruscations of the aurora borealis. Suppose then, that a fluid in all other respects similar to the *electric* is formed on that side of a comet which is turned from the Sun; a fluid so rapid in its rectilinear motion, that it soon gets out of the sphere of gravity of that body, without being detained by its atmosphere, and which is rendered *luminous* by its decomposition on its course: thus we shall have the phenomenon of the *tail*, with a circumstance characteristic of these causes, namely the *curvature* of these tails, the *convexity* of which is turned toward the side to which they are moving; the cause of which appears to me to be as follows.

Aurora borealis.

Electric fluid.

Lightning.

Tails of comets a similar fluid.

Cause of their curvature.

The particles of the fluid of the tail, as they are detached from the comet, possess the same *projectile* movement with it, and in the same direction. Accordingly they must continue to follow it. But if they extend very far, that is to say, if the tail become very long, the particles that proceed the farthest, continuing to move with the same velocity but in a larger orbit, must have a less *angular* movement. This must

must produce such a *curvature*, not to mention some degree of *resistance*, which these particles may experience in *space*, though with respect to large bodies it is imperceptible. Finally, the *tail* will extend so much farther to our eyes, in proportion as the production of this fluid is more abundant, and its *decomposition* more slow; both which circumstances may vary, not only in different comets, but in the same comet at its different returns. Combining these different considerations therefore, may it not happen, that a comet observed with a very long tail, and of considerable apparent diameter, from the greater spread of its envelope of *luciferous vapours* to a determinate distance from its body, shall return with very little tail, and a much less apparent diameter; that its light even shall be weaker; and that thus it shall not be known for the same as was expected from calculation, though in reality it is so?

Here let me stop. He who coasts along the shore by the help only of the lead and the log, without a compass and without a quadrant, must not set up for a geographer, but confine himself to hints. In conjectures like the present, the analogies on which they are founded can furnish only germes of a theory: time alone can show by repeated observations, whether they be capable of becoming fruitful; but at least they afford matter for reflection and inquiry, and the first ideas I communicated to you have already been corrected and extended by your remarks. At your desire I now deliver them to you, such as they are, in testimony of my respect, &c. Conclusion.

Berlin, 2 *June*, 1799.

XII.

An Example of the Utility of a Series in finding a Fluent. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IT sometimes happens, that great mathematicians give themselves no inconsiderable pains to untie a knot, which they might cut with the greatest ease. As an instance of this Difficulty of direct solutions.

this, I beg to offer you a determination of a fluent, which seems to have been considered as presenting some difficulties scarcely to be overcome.

The equation to be resolved is this; $\int x y \dot{x} \surd (\dot{x}^2 + \dot{y}^2) = m x \dot{y}$: hence, by squaring both sides, we have $[(\int x y \dot{x})^2 - m^2 x^2] \dot{y}^2 + (\int x y \dot{x})^2 \dot{x}^2 = 0$, and $[(\int x y \dot{x})^2 - m^2 x^2] \frac{\dot{y}^2}{\dot{x}^2} + (\int x y \dot{x})^2 = 0$. Put $y = a + b x^2 + c x^4 + d x^6 + \ldots$, then $\int x y \dot{x} = \frac{1}{2} a x^2 + \frac{1}{4} b x^4 + \frac{1}{6} c x^6 + \ldots$; of which the square may be called $A x^4 + B x^6 + C x^8 + \ldots$: again, $\frac{\dot{y}}{\dot{x}} = 2 b x + 4 c x^3 + 6 d x^5 + \ldots$, the square of which may be called $\alpha x^2 + \beta x^4 + \gamma x^6 + \ldots$; then, by multiplication and substitution, our equation becomes $(A - \alpha m^2) x^4 + (A \alpha + B - \beta m^2) x^6 + (A \beta + B \alpha + C - \gamma m^2) x^8 + \ldots = 0$; whence, by the general law of infinite series, $A = \alpha m^2$, $A \alpha + B = \beta m^2$, $A \beta + B \alpha + C = \gamma m^2$, ..., and $\alpha = \frac{A}{m^2}$, $\beta = \frac{A \alpha + B}{m^2}$, and so forth; but, by actual involution, we find $A = \frac{1}{4} a^2$, $B = \frac{1}{4} a b$, $C = \frac{1}{6} a c + \frac{1}{16} b^2$, ..., and again $\alpha = 4 b^2$, $\beta = 16 b c$, $\gamma = 24 b d + 16 c^2$, ..., whence $b = \surd \frac{\alpha}{4}$, $c = \frac{\beta}{16 b}$, $d = \frac{\gamma - 16 c c}{24}$, $e = \frac{\delta - 48 c d}{32}$, and so forth; and finally, by reduction, we have $b = \frac{a}{4 m}$, $c = b^3 + \frac{b}{16 m}$, $d = 2 b^5 + \frac{5 b^3}{18 m} + \frac{b}{576 m^2}$, and $e = 5 b^7 + \frac{35 b^5}{32 m} + \frac{41 b^3}{1152 m^2} + \frac{b}{36864 m^3}$; and in the case where a becomes extremely small, so that its higher powers may be neglected, $c = \frac{b}{4^2 m}$, $d = \frac{c}{6^2 m}$, $e = \frac{d}{8^2 m}$, and $f = \frac{e}{10^2 m}$. If, in particular cases, beside the general equation, we have a determination of the value of $\int x y \dot{x}$, for instance $\int x y \dot{x} = n x$, we may obtain an equation between x and a, whence either of them may be found from the other; that is, $\frac{1}{2} a x + \frac{1}{4} b x^3 + \frac{1}{6} c x^5 + \ldots = n$: hence, if x becomes very small, $a = \frac{2 n}{x}$; and if a is evanescent, $\frac{a}{2}\left(x + \frac{x^3}{2 \times 2^2 m} + \frac{x^5}{3 . 2^2 . 4^2 m^2} + \frac{x^7}{4 . 2^2 . 4^2 . 6^2 m^3} + \ldots\right) = n.$

For

For example; let $m = \frac{1}{200}$, and $n = \frac{3}{800}$; and let a be first $\frac{1}{200}$: then b will become $\frac{1}{4}$, $c = 3{\cdot}14$, $d = 20{\cdot}5$, and $e = 76{\cdot}7$; and supposing $x = {\cdot}3$, the equation for n will become $\frac{3{\cdot}6}{800}$ by the correct series, and $\frac{3{\cdot}44}{800}$ by the approximation: then in order to reduce n to its true value, we may may make $x\,y\,x' = (n\,x)' = x\,n'$, very nearly, and $x' = \frac{n'}{y}$; Examples.

but y is found, in this case, ${\cdot}074$, and $x' = \frac{{\cdot}6}{800 \times {\cdot}074} =$ ${\cdot}01014$, whence x is accurately ${\cdot}2898$: or, x being ${\cdot}3$, a will be nearly $\left(\frac{3}{3{\cdot}44}\right) \times {\cdot}005 = {\cdot}0044$, by the approximation. In the second place, let a be $\frac{1}{20}$: then b will be $\frac{5}{2}$, $c = 46{\cdot}875$, $d = 1237$, and $e = 47200$; and suppose $x = \frac{1}{9}$; then n becomes ${\cdot}00382$, $n' = {\cdot}00007$, and x corrected ${\cdot}1104$. For a third example, if x be $\frac{1}{4}$, we may find an approximate value of a by the second series, which is ${\cdot}0156$; and this being necessarily somewhat too large, we may assume $a = {\cdot}015$, whence n is found ${\cdot}00371$, and n' being ${\cdot}00004$, a' will be nearly $\frac{a}{n}n'$, or ${\cdot}00016$, and a, corrected, ${\cdot}01516$. In the first and third examples, the calculations of an eminent mathematician on the continent, deduced nearly from the same data, have given for a, instead of ${\cdot}0044$ and ${\cdot}0152$, ${\cdot}0038$ and ${\cdot}0136$; while some very accurate experiments had long before made the results ${\cdot}005$ and ${\cdot}015$.

If we wished to investigate the properties of the returning branch of the curve, of which x and y are the coordinates, it would be necessary to apply a proper correction to the fluent $\int x\,y\,\dot{x}$; another form of a similar curve might also be obtained by substituting $p - y$ for y; and when x becomes infinite, we find, by a different mode of calculation, beginning from the other end of the curve, $-x = \sqrt{(p^2 - y^2)} + \frac{p}{4}\,\mathrm{H.L.}\,\frac{p - \sqrt{(pp - yy)}}{p + \sqrt{(pp - yy)}}$. Other cases.

I am, Sir,

Your very obedient servant,

13 *Feb.* 1809. E. F. G. H.

—VII.

VII.

An Account of the Method of cultivating the American Cranberry, Vaccinium Macrocarpum, *at Spring Grove. By the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c***.

Cranberry cultivated with success.

THE American cranberry, the *vaccinium macrocarpum*, has for some years been cultivated with success at *Spring Grove*, and as the fruit of it is now become an object of some importance in the economy of the family, a short account of the management of this unimproved plant will it is to be hoped prove acceptable to the Members of this useful Society, and not uninteresting to the Public at large.

Supply from water.

For the better understanding the intended communication it is necessary to premise, that a spring rises in a small grove within the precincts of *Spring Grove*, which is no doubt the origin of the name; this spring is carried in leaden pipes into the house, to which it affords an ample supply; the waste water is suffered to run through a small basin and a pond in the pleasure ground, before it escapes to *Smallberry Green;* to this constant supply of fresh water, though it is very small, the great luxuriancy, with which water plants of all kinds suitable to this climate succeed in the pond, is no doubt in some degree to be attributed.

Artificial island.

In the middle of the basin, a small island has been formed, by supporting a box of oak upon posts driven into the bottom; in the centre of this pond, the waste water which used before to issue through a fountain, is suffered to flow in the form of a spring, which rises into a large shell of the *chama gigas*, perforated for the purpose, imitates very well a natural spring, and gives in hot weather an appearance of freshness and coolness, very pleasant to those who walk in the garden.

The oak box, which constitutes this artificial island, is circular, 22 feet in diameter, and 13 inches deep; the bottom is 5 inches under the surface of the water, and bored

* Trans. of the Horticultural Society, vol. I, p. 75.

through

through with many holes; on this a layer of stones and rubbish was first placed, and upon that a covering of bog earth, brought from *Hounslow Heath*, which together are 5 inches below and 7 inches above the surface of the water of the basin; in this bed of black mould, a variety of curious bog plants were placed about seven years ago, which flourished in an unusual degree, among these was the *vaccinium*, which flowered and ripened its fruit the first year.

Bog plants.

Cranberry.

In the autumn of the second year it again produced a plentiful crop, and soon after began to send out runners somewhat resembling those of a *strawberry*, but longer, and rather less inclined to take root while young; they did however take root in the winter, and early in the spring threw out upright branches 10 inches and a foot long, on which the flowers and fruit were chiefly placed; the produce was this year gathered, and found to be high flavoured berries, very superior to those imported, which have in general been gathered unripe, and have become vapid and almost tasteless by long soaking in the water, in which they are packed for carriage.

Runners.

Fruit much superior to that imported.

It was now determined to consider the *American cranberry* as an article of kitchen garden culture, and to give up the whole of the island to it, which in a few years it entirely covered by its own runners, without any fresh plants being put in, and this bed, with the addition of some hanging boxes receding from the centre to the sides, produced, in the year 1806, 23 bottles of very fine *cranberries*.

Cultivated for the kitchen.

In the year 1805, a bed was made on the side of the pond 20 feet long and 5½ feet broad, by a few stakes driven into the bottom parallel to the side, and lined with old boards; the bottom of this was filled up with stones and rubbish, and on these a bed of black mould, 3 inches above and 7 inches below the usual surface of the water, was laid: this was planted with *cranberry* plants, many of them having been rooted in a hot bed, in which they thro e most vigorously. In this autumn, 1807, the bed produced a crop, which, added to that of the island, afforded a supply for the family of 5 dozen bottles of cranberries, beside a small basket reserved for present use. The total contents of the two cranberry beds is 326 square feet; the quantity of land em-

Extended to a bed by the side of the pond.

employed

ployed for raising *strawberries* at *Spring Grove* is, after the divisions between the beds have been deducted, 5645 square feet; the beds necessary to give a sufficient supply of *cranberries* for the family did not therefore occupy quite $\frac{1}{8}$th of the space allotted to *strawberries*.

Accident suggests many useful hints.

The society will, I hope, forgive this detail of the origin and progress of this kind of cultivation: successful as it has been, it must still be considered in its infancy, and not sufficiently established to afford general rules for the regulation of a gardener's proceedings: it originated entirely in a fortunate accident, the history of which, will, it is hoped, give an adequate idea of the method now practised, and at the same time bear testimony in favour of the opinion, that more benefit has been derived in the advancement of horticultural knowledge, by pursuing the hints which nature continually gives, than from the effects of abstract reasoning and original invention.

The crop not liable to injury.

It is remarkable, that, during the seven years these *cranberries* have been cultivated at *Spring Grove*, no circumstance has arisen, from the variety of seasons, from blight, or any other circumstance, that has diminished the quantity of a full crop; the flowers have issued out of their buds, in abundance, in their due season, and fewer of them have been abortive, than in general is the case in other plants. The fruit has gradually swelled and duly ripened without being subject to the attack of any vermin, or to injnries of any kind from the excesses of heat or cold, or from those of wetness or of drought.

VIII.

On the Existence of Animal Matter in Mineral Substances. From Parkinson's Organic Remains.

Marble tinged with the colouring matter of coral.

IN a marble formed by a species of tubipore resembling the tubipora musica a reddish tinge is observable, which evidently proceeds from some of the original colour of the coral having been preserved. This is rendered indubitable by a close examination of the specimen itself, since it is there seen,

seen, that the colour does not exist in the intermediate calcareous matter, or in that which has been introduced into the cavities of the tubes, but in the substance of the coral, forming the sides of the tubes.

It appears, from the experiments of Mr. Hatchett, that the colouring matter of the organ-pipe coral is similar to that of the red coral, *(gorgonia nobilis)* being some unknown modification of animal matter. The red colour of both these substances having been gradually destroyed, during the action of the diluted nitric acid, as the solution of the calcareous substance advanced; and could not afterward by any means be restored: nor could any colouring principle whatever be detected by the reagents usually employed*.

This some unknown modification of animal matter.

We also learn from the Count de Marsilli, that by digesting red coral, with or without its membranaceous tunic, in milk, over a slow fire, the colouring matter was dissolved by the milk, which became thereby of a pink colour; while the coral became first of a saffron, then of an ash colour, and at last of a livid white. The same effects resulted from its digestion in heated wax. A similar deprivation of colour is also found to take place in those species of coral, which, having been broken, have fallen into, and have remained for some time in the mud at the bottom of the sea†.

Solution in milk,

in hot wax,

and in mud of the sea.

As the colouring matter of these corals is capable of being thus removed by digestion, it is not to be wondered at, that, in general, in the fossil specimens, which must, in most instances, have been exposed to long continued maceration in water, little or none of the original colour remains.

Thus most fossils deprived of it.

From the weight and other physical characters, as well as from exposure to chemical agents, I found, that every fossil of this species in my collection, not imbedded in stone, contained too much siliceous matter, to admit, by the agency of an acid, an examination into the change, which had taken place in the original constituent parts of the coral.

Separate fossils too siliceous for such an examination.

Indeed, considering that in the experiments of Mr Hatchett even the recent tubipore lost its colour, and only de-

* Philosophical Transactions, for 1800.

† Histoire Physique de la Mer, par Louis Ferdinand Comte de Marsilli.

monstrated

monstrated some loose particles of a tender membrane, I regarded it as almost hopeless to attempt to detect any animal matter in a fossil body, which must have existed in a mineralized state several thousand years; but as the result, if successful, would prove highly interesting, I resolved on the experiment.

A piece of the marble digested in dilute muriatic acid,

A fragment of the marble was therefore exposed to the action of the muriatic acid, in a very diluted state. As the calcareous earth dissolved, and the carbonic acid gas escaped, I was much pleased to observe the membranaceous substance appear, depending from the marble, in light, flocculent, elastic membranes. Many of these most unexpectedly retained a very deep red colour, and appeared in a beautiful and distinct manner, although not absolutely retaining the form of the tubipore.

and animal membrane demonstrated.

Another instance of this.

Another curious specimen of marble exhibits a similar instance of animal membrane preserved for ages from destruction by being enchased in stone. This marble is of a brownish red colour, much darker in its interstitial parts than in the corallites themselves, and is susceptible of a good polish. The madrepore, which has entered into the formation of it, appears to have been of the smaller species, the branches being about the size of a goosequill.

Its colouring matter.

The diffusion of the red colour through this specimen requires some little attention. It extends through every part of the mass, and appears to have percolated through the external part of the coral into its internal substance, in such a manner that no gross substance has entered; the colouring matter appearing to have been here deposited from its solution. That the colour has not been derived from the coral itself, may safely be inferred: there appear therefore to be two modes by which this colour may have been yielded. Either the decomposed coral might have been imbedded in a matrix, of which the oxide of iron formed a part, and which by the access of a fit menstruum became capable of penetrating through every part of the coral: or both the coral and the surrounding matrix might derive their colour from the influx of the coloured fluid derived from some other source. In either case, it appears evident; that this diffusion of the colouring matter, and its introduction into the

the mass, were previous to the perfecting of the lapidifying process.

On subjecting this marble to the action of diluted nitrous acid, its decomposition took place very speedily. A considerable quantity of carbonic acid gas was separated, the calcareous part was dissolved, and a red substance, an oxide of iron, gradually sunk to the bottom. While the decomposition was proceeding, a substance was detected, the presence of which, at least in so obvious a state, was not expected. As the separation of the other parts took place, ragged flocculent pieces of apparently a membraneous substance were left, adhering to those parts where the coralline substance had been observable. These, on the least agitation of the fluid, were seen to wave to and fro, and on the motion being increased fell off, and soon reached the bottom of the vessel from the weight of the solid matters which were attached to them.

Nitrous acid detected oxide of iron,

and exposed a membrane.

In the specimens of marble from Kilkenny, the remains of a coral of this kind, but of a larger species, are very evident. The ground of the marble is of a deep black, but the part of the marble possessed by the coral is of a very light gray. In fineness of grain and in susceptibility of polish it appears to equal any marble. From this circumstance, and from the considerable difference in the colour of the madrepore and of its matrix, the structure of the former becomes very conspicuous, and the astonishing labours of its original inhabitant are very easily traced. In one part, the converging perpendicular plates, displayed by a horizontal section of the madrepore, are discovered; while in another part a longitudinal section has not only shown numerous horizontal plates; but also yields a fair view of the beautiful reticular texture of the coral, resulting from the frequent intersections of the perpendicular by the transverse lamellæ.

Marble from Kilkenny.

The very considerable difference of colour in the ground of this marble and in the animal part is particularly deserving of attention. As in the former specimen the regular diffusion of colour through the whole mass appeared to authorize the conjecture, that the colouring matter was introduced previous to the coral having undergone its lapi-

Appears to have undergone two petrifying processes.

deous

deous change; so here, the exclusion of the blackening particles from the coralline part of the marble seemed to warrant the supposition, that the coral had acquired a stony impregnation previously to its having become imbedded in the including mass of calcareous matter. Thus two distinct lapidific processes, occurring perhaps at the distance of many ages, may have been employed in forming the marble of which we are now treating. Any difficulties which appear to be in the way of this supposition will diminish, when it is considered, that in several marbles, indeed in all the *breccia* marbles, this twofold lapideous impregnation must necessarily be admitted. These are composed of fragments of various marbles, which, after having been formed in perfect strata, have been broken into small pieces, and have then become agglutinated into a compact mass, by the medium of a fluid, which, from its saturation with the carbonate of lime, has possessed the required lapidific power, which it has exerted during its interposition between these detached fragments.

Breccias.

The marble digested in dilute muriatic acid.

A piece of this marble was suspended in a glass vessel, containing diluted muriatic acid, and was speedily dissolved with effervescence. During the decomposition of this piece of marble, not the smallest filament of membranaceous substance became detached; but, on the contrary, the newly forming surface was as perfectly clean and smooth, as if it had been a piece of primitive limestone: the black matter from which the marble derived its colour falling to the bottom of the vessel, during the solution of the marble. This powder being dried was projected on melted nitre, and immediately produced deflagration: a circumstance which, with the form of the coral having been visible in the marble, shows the curious fact, that a part of the colouring matter of the marble was an animal charcoal.

No membrane appeared,

but an animal charcoal.

A discoid corallite exhibited animal membrane.

A corallite of the discoid kind was suspended in muriatic acid much diluted, which, by removing the calcareous earth, soon exposed the flocculent membranes of the madrepore.

But, in this instance, the membraneous flocculi were exceedingly small, hanging but a very little way below the ridge of the coral; and, on the least agitation of the glass, innumerable minute portions of membranes became detached,

tached, and slowly sunk to the bottom of the glass. The smallness of the pieces of membrane might be here probably accounted for by the structure of the madreporite which they composed; since from the multitude of pores with which it is pierced, the membrane must suffer such frequent inflexion, as would give very little reason to expect, that, in a corroded preparation of even the recent madrepore, any thing like the form of the madrepore could be preserved. The circumstance of these membranous flocculi appearing to possess a greater degree of gravity than belongs to animal membrane in general may be accounted for, by considering, that they may yet retain some particles of earth in some of their cavities, which are defended from the action of the acid, by being completely enclosed in the membrane.

A very perfect membrane obtained from a spongite.

A small, light coloured, calcareous spongite, or alcyonite, possessing somewhat of a conical form and a rugose surface, and exhibiting, when examined with a lens, evident marks of an original spongeous substance, was suspended in water slightly acidulated with muriatic acid. As the acid acted on the carbonate of lime, the membraneous part of the sponge began to appear; and being liberated, extended itself beyond the remaining solid mass in tolerably coherent, flexible flocculi. When it had been submitted so long to the action of the acid, that the carbonate of lime was nearly removed, a dark brown reticulate mass was left, bearing the general form of the fossil, and manifesting its original spongeous reticulated texture. So coherent was this mass, as even to bear its removal, by pouring into another glass, without suffering any material injury to its form. It was however at last broken, upon pouring additional water into the phial which contained it. Another specimen, as similar in its form and size as could be found, was then subjected to the action of the acid for the purpose of obtaining a correct sketch of the remaining membrane, and with equal success.

IX.

IX.

Observations on the different Species of Dahlia, *and the best Method of cultivating them in* Great Britain. *By* R. A. Salisbury, *Esq. F. R. S**.

Dahlias.

NO flowers, which have been lately introduced into the gardens of this island, are more showy than the *dahlias*; and they possess the additional merit of being produced at a season, when most others are decaying; nevertheless, it will appear in the subsequent pages, that by a little management these plants may be made to blossom at a much earlier period; and that in vallies, or low situations, where our autumnal frosts frequently cut them off early in *October*, it is the only method of obtaining their flowers at all. I am more emboldened to offer these results of my own experience to the Horticultural Society, as they have turned out very different to what was expected from the hints thrown out upon this very subject by one of the first gardeners in the world.

History of them.

First mentioned by Hernandez.

The earliest account I am able to trace of these plants, which are all natives of *Mexico*, is in Hernandez' History of that country, published in 1651, where two species are figured. He says that the first grows in the mountains of *Quauhnahuac*, and is called *acocotli* by the inhabitants; that it has leaves composed of five leaflets, some of which are sinuated, slender peduncles, with pale-red stellated flowers; that the roots are tuberous, strong and bitter in taste, and, according to the fashionable jargon of his time, hot and dry in the third degree; that an ounce in weight, taken internally, is a powerful medicine, alleviating pains in the bowels, expelling flatulence, increasing the urinary discharge, promoting sweat, strengthening cold languid stomachs, excellent against the colic, resolving obstructions, and dissipating tumours if externally applied. This is clearly the pale red variety of *dahlia sambucifolia*. The second he calls *acocotli ligustici facie*, but gives no descrip-

* Trans. of the Horticultural Society, vol. I, p. 84.

tion

tion of it: the figure however, though destitute of flowers, leaves no doubt, that it is the species called *dahlia bidentifolia* in *Paradisus Londinensis,* and from the size of its foliage most probably the orange coloured variety.

Mr. Thiery Menonville, in the interesting detail of his journey to *Guaxaca,* published in 1787, is the next author, who to the best of my knowledge has noticed any species of *dahlia.* It is well known, that this botanist was employed by the French Minister, to steal the *cochineal* insect from the Spaniards. In this dangerous mission, he tells us, that having entered one of the gardens in the suburbs of that city, adjoining to a plantation of *nopals,* upon which the insect feeds, he was struck with the beauty "*d'une aster violette et double, aussi grande que celles de France,* "*mais produite par un arbuste tres semblable pour les feuilles* "*pinnées à notre sureau.*" From the violet colour of the flower, I am inclined to think, that this is the species which I have called *dahlia sphondyliifolia.* T.Menonville.

The third author, who has written upon these plants, is the late Abbé Cavanilles. From a semidouble variety of *dahlia sambucifolia,* which flowered at *Madrid* in *October* 1790, he in the first volume of his *Icones,* published in the following *January,* first defined the characters of the genus scientifically, naming it in honour of Andrew Dahl, a Swedish botanist, with the specific title of *pinnata.* Afterward, in the third volume of the same work, he makes us acquainted with two more species; his *rosea,* which from this ambiguous title has been confounded both here and at *Paris* with his first; and his *coccinea,* no less absurdly so denominated, its ligulated florets varying from yellow to orange, but never assuming a scarlet tint. My reasons for adopting his generic, but none of his specific names, will be given hereafter, and are conformable to the usage of Linne in those classical works *Flora Lapponica* and *Hortus Cliffortianus.* Cavanilles. Name.

These three *dahlias* having been sent to *Paris* from *Madrid* by Cavanilles in 1802, a very ample memoir with coloured figures was published two years afterward by Monsieur Thouin, in that celebrated national work, the *Annales du Museum d'Histoire Naturelle,* and he makes the fourth Thouin.

Description of the plant.

writer upon them. We there learn, that they are perennials, losing their stems at the approach of winter, which do not push forth again till late in spring; that their roots consist of fleshy tubers disposed like those of the *asphodel*, though less numerous; that on their arrival they were planted in large pots of substantial earth, and protected from frost under a frame; that the stems grew little till the great heats of summer commenced, when they lengthened rapidly, and flowered in the end of autumn. Monsieur Thouin then describes the first, his *dahlia rose*, as attaining seven feet in height; leaves opposite, composed of from 5 to 9 leaflets; flowers about the size of a *China aster*; ligulated florets commonly 8 in number, pale red inclining to flesh colour; of which all the earlier flowers ripened seeds. The second, his *dahlia ponceau*, was only four feet high; stem slender, covered with a fine meal; leaves doubly pinnated, and pale green; flowers smaller than in the two other species; ligulated florets from 8 to 9, red orange colour; this flowered later, and did not ripen seeds. The third, *dahlia pourpre*, he erroneously supposes to be Cavanilles' *dahlia pinnata*, and thinks it greatly superior in beauty to both the others; the roots of this he observes are covered with a violet coloured cuticle; stems about five feet high; leaves often produced in threes; flowers semidouble; ligulated florets of a rich violet purple, approaching that of the *pansy*, or still more like the fruit of the *prune de Monsieur*, which on their inner surface reflect the light variously *(chatoyante)* like a shot silk; it flowered the latest, and only ripened very few seeds.

His mode of cultivation.

After paying some handsome compliments to Cavanilles for sending, and to Dr. Thibaud for bringing the roots from *Madrid*, this candid and judicious gardener proceeds to state what, he conceives, will be the properest mode of treating these plants. He remarks, that, being newly arrived, with all the original habits contracted in their native climate, it will only be after a lapse of years, that their culture can be thoroughly understood; and that if he anticipates any directions on this head, it is rather to excite the attention of others to the subject, than lay down fixed and positive rules for their conduct. From the magnitude of the

the roots, the abundant foliage, and rapid expenditure of sap in these plants, he concludes, that a strong but very rich soil, nearly such as *orange* trees delight in, will be most suitable for them, with plenty of water in dry weather. Being ignorant of their particular locality in *Mexico*, he doubts whether they will live through winter at *Paris* in the open ground, giving his opinion in the negative, for the following reasons: 1*st*, herbaceous plants so tall and tender are seldom met with in high mountains, the dominions of winds, snows, and storms; 2*dly*, these plants, when exposed to a temperature of 7 or 8 degrees below zero, turn yellow and sick; 3*dly*, they are late in beginning to vegetate, and require a long protracted autumn to expand their flowers: 4*thly*, their roots had been already killed at *Paris*, by a frost of five degrees in one night. Notwithstanding this unfavourable statement, Monsieur Thouin does not despair of being able in time to change their habits, and acclimate them in *France*: to this end, he proposes forwarding them in spring with a little artificial heat, and wisely remarks, that our days in summer being longer than in *Mexico*, a sufficient maximum of heat to bring their flowers and seeds to perfection may thus be obtained; that thus *barley*, which in the north of *France* requires six months to ripen it, in *Russia* is often perfectly matured in forty days. He then brings instances of two plants from the same country, the *marvel* of *Peru*, and *long-flowered marvel* of *Peru*, which, though very tender when first introduced, are now become more hardy, the former especially often springing up in their parterres from self-sown seeds. Lastly, he informs us, that all the *dahlias* may be increased by seeds, dividing their roots, or even by cuttings of their stems, though that part is annual; but seedling plants, he remarks, do not flower the first year, and the memoir concludes with some general remarks on the beauty, and ornament, which they will add to our borders or conservatories, in autumn.

Longer days equivalent to greater heat in vegetation.

Mode of propagation.

The fifth author upon this genus is Professor Willdenouw, who in his *Species Plantarum* most unwarrantably changes its name, under the pretext that another *dahlia* was already established in *dioecia!* This is so far from being true, that the description of the *Cape* plant he alludes to,

Willdenouw.

by

by Professor *Thunberg*, in the *Skrivter af Naturhistorie Selfskabet*, 2 *bind*, did not come out till 1792; nor was the manuscript even read before that society, till *April*, 1791, three months after Cavanilles' *dahlia* had been published. I am aware, that there is no general rule without an exception, and that in some cases the right of priority must be given up; but in this not a shadow of reason for the innovation can be offered, and as these plants are universally known, both in our Island, and upon the Continent, by the name of *dahlia*, much inconvenience will ensue for a time, if the other be adopted: moreover, it would be unjust to Cavanilles, who is dead and gone.

I must now venture to give some account of the introduction of the *dahlias* into our own island; when it will appear how rapidly we have improved upon the French method of treating them; and as they have already not only produced a number of varieties with us, but each species requires a somewhat different management, I shall offer such observations, as I hope will not be found quite useless respecting them.

1st species. Introduction into Britain.

The first species, *dahlia sambucifolia*, was introduced into this country by the Right Honourable Lady Holland, who sent the seeds from *Madrid* in *May*, 1804, which have produced several varieties of different colours. For this reason, I have rejected both the specific names *rosea* and *purpurea*, for one that is applicable to both of them. *Pinnata*, as Monsieur Thouin observes, is equally inadmissible, because many of its leaves only consist of three leaflets; and a greater number of the leaves of the 2d species being also pinnated, it has already occasioned much confusion. Though the seeds arrived so late in this country, several of them flowered the succeeding autumn at *Holland House*, and the variety ζ with deep purple flowers was immediately pretty well figured in the *Botanist's Repository*. By the constant attention of Mr. Buonaiuti, in pressing out the moisture, which is collected among the florets after the calyx closes, a number of seeds were ripened in 1805, and some of these were liberally communicated to me late in the month of *April*, 1806.

Specific name.

Culture.

I had no opportunity of sowing them till the 5th of *May*, when they were put into two pots of light rich earth, plunged

plunged to their rims in a bed of dung, which had nearly lost its heat, having been made two months. A dozen plants soon came up; and on the first of *June*, being about 5 inches high, as well as very stiff, from throwing down the glasses in the day time, were transplanted into separate pots of $2\frac{1}{2}$ inches diameter. In these they continued three weeks, when two of the strongest were removed without breaking any of their fibres into large pots of very rich mould, with the intention of following Monsieur Thouin's directions minutely; five of them into pots one size larger, of very rich mould, and five of them into pots one size larger, of poor sandy mould. All these plants were twice more transplanted into somewhat larger pots before the 10th of *August*, by which time the two largest were 4 feet high, and the others not much shorter, though less branched. They were now all removed from the hot-bed frame, having been exposed to the open air both night and day the last month; the two largest into a border of rich earth, but the rest plunged as they stood in the pots, in various parts of the garden, near the walls, but only in west and east aspects, that to the south being entirely filled with other plants. Their stems and branches, as they advanced, were carefully secured from being broken by the winds, and they were supplied with water, whenever their leaves flagged. They all grew rapidly in *August* and *September*, but I despaired of seeing any flowers till the middle of the latter month, when almost every branch terminated in a flower, the first of which opened the 7th of *October*. Soon after others came out; but what is well worthy of attention, the two largest plants, which had been nourished the most luxuriously, though placed in the warmest corner, were the latest in showing flowers. One of these, which had attained to twelve feet in height, did not expand its first flower till the 29th of *October*, producing however a plentiful succession till the beginning of *December*; in the first week of which a violent storm of wind and rain nearly put a stop to its vegetation. All the plants ripened seeds more or less, and were suffered to remain in the ground with their decaying stems uncut, till a frost came, which was severe enough to freeze the borders an inch deep. The morning after, Flowering.

those

those which had been plunged in their pots were taken up and removed into the greenhouse, behind other plants: the two in the ground, after cutting down their stems to about a foot and a half in length, and removing the frozen crust of earth, were protected with a covering of moss and fern about six inches thick.

Management the second year.

In 1807, the greenhouse plants were removed into the open air so early as the 27th of *April*; and the *dahlias* at the same time just beginning to push were turned out of the pots, and planted in very different parts of the garden, as well as in very different soils. Having observed the preceding year, that those which had been confined in the smallest pots and poorest earth not only flowered the earliest, but made to a gardener's eye the handsomest plants, being only from 5 to 6 feet high, with scarcely any branches, and panicles of from 7 to 13 flowers; I ordered some of them to be placed in pure gravel from which all the larger stones had been screened, others in a dry seam of sand which crossed the garden, and others again in rich earth; they were all supplied with water however during the dry part of the summer.

Seedling plants.

Beside these, a number of seedling plants were distributed at random in different gardens; and what gave me no little satisfaction, I observed in *June*, near the large plant of all, a cluster of young seedlings coming up from a head which had been supposed rotten, and dug into the border at its winter cleaning.

Flourished best in screened gravel.

The result in autumn was very similar to that of the former year, but with a still more decided advantage to the plant in screened gravel. One of these expanded its first flower the 19th of *August*, and its last the 27th of *September*; all the seeds ripening perfectly: being the dark purple variety ζ, and planted singly in the middle of an open grass plat, it attracted far more attention than the venerable *chestnuts, magnolias, Cembra pines, cedars,* and *cypresses*, relicts of Peter Collison's labour, which surrounded it. The largest plant of all, against the south east side of the house, in rich earth, had not opened a single flower on the 14th of *October*, when I left the place; but though plants at *Holland House*, as well as in Messrs. Lee and Kennedy's garden at *Hammersmith*, had then been already blasted by the frosty nights, I understand it remain-

ed

ed uninjured, and continued blowing till the middle of *November*, in great beauty.

It is necessary to observe, that the Village of *Mill Hill*, where I lately resided, is situate upon a high ridge, at the head of two vallies, in which some of the sources of the little brook, called the *Brent*, arise; and the garden, in which these *dahlias* were cultivated, is well screened from the weather by high trees. Being rather above the level at which the exhalations of the adjacent country pass off, the early autumnal and late spring frosts never reach it; at least they have been so mild during the six years I lived there, as never to have cut off *cucumber plants, potatoes, french beans,* and *tropæolums*, till long after others of the same species had been killed in the vallies. In hoar frosts, the top of *Harrow Hill, Bushy Heath, Elstree*, and *Totteridge*, are commonly seen green, or illuminated by the sun, when the rest of the neighbourhood is white as snow, or obscured in a sea of fog. The medium temperature of this delightful spot, and I believe of most other grounds equally elevated, during the months of *December, January* and *February* is considerably milder than in any valley, perhaps never less than from 1 to 5 degrees: in extremely severe frosts, the difference is still more apparent, so that when the cold has been down to 12 and 9 degrees of Fahrenheit's thermometer in *London*, it has only been 20 and 16 there; and this is likewise proved by the more tender exotic plants still remaining in the garden, some of them 60 and 70 years old. The common broad leaved *myrtle* against a wall there quickly grows to 6 feet in height without any covering, and the *cupressus sempervirens*, as well as *arbutus unedo*, are rarely scorched, and never killed. The summer temperature of *Mill Hill*, on the contrary, is as much cooler than that of the vallies, as its winter temperature is milder; and it suffers greatly in dry seasons from the want of those dews, which refresh the latter; both circumstances unfavourable to the success of such perennial plants as the *dahlias*: nevertheless they have apparently succeeded better here than in any other place. No intelligent gardener, after reading the foregoing detail, can be at a loss how to treat these plants, nor is there the smallest doubt, that by check-

ing

Mill-hill.

High grounds warmer in winter than valleys,

but cooler in summer.

ing the luxuriance of the herbage, their flowers may be brought to perfection, even in situations the most exposed to autumnal frosts. I have only one caution to give, which is, that in whatever soil they are planted, but especially if it is poor and gravelly, they must be duly watered in dry weather, till the flower bud can juſt be discovered in the heart of the leaves; after which they will require none whatever. Insects do not appear to attack them much, except the *ear-wig*, and for this I know no remedy but personal labour in catching and destroying them: that their numbers and consequent havoc however may be wonderfully diminished in the course of a few years, by moderate exertions, I have twice experienced.

Caution.

Earwigs.

2d and 3d species.

Their introduction.

The seeds of the second and third species, *dahlia sphondyliifolia*, and *bidentifolia*, were also sent along with those of the first, from *Madrid*, in *May*, 1804, by the Right Hon. Lady Holland: but one plant of the *dahlia sphondyliifolia* had been previously introduced from *Paris*, by E. J. A. Woodford, Esq., and flowered in his garden at *Vauxhall* in the autumn of 1803. The same seasons for which I have changed the specific names of the first species oblige me to offer new ones for these two. Cavanilles' names are actually false: for no variety of the second has yet been seen with rosecoloured florets, or of the third with scarlet; so that on this account Dr. Sims has very justly hesitated to quote him in the *Botanical Magazine*; and a yellow variety having since been introduced, it is become doubly objectionable. Both these species are more tender and flower later than the first, so that they require with us every help which art can give to forward them in spring. The best method I can suggest, and which succeeded at *Mill Hill*, is to keep them always in pots, except a plant is wanted to be much branched for making cuttings. After the first day of *April*, accelerate their growth in a very airy frame, exposed as much as possible to the light, but with very little or no bottom heat from dung; and about the middle of *June* plunge the pots close to a south east or south wall, nailing up the branches as they shoot. All the secondary branches should be pinched off while tender, with the finger and thumb, and even their principal leaves partly cut off, if the plants

Specific names.

These more tender.

Management.

plants are disposed to be very luxuriant. Just water enough to keep them growing must be given daily, but no more. I surely need not add, that in such heavy rains as sometimes fall here after the summer solstice, they must have no water at all; but rather, if possible, be protected from their violence by a glass light.

X.

Some Questions in Ornithology. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

AS I know not where an inquirer will be more likely to meet with information on any points of natural history than from the many scientific readers of your valuable Journal, I should take your inserting the following as a favour, and would be much obliged to any of your readers, who would be so kind as to elucidate the point in question. Queries respecting

I am, Sir,

Your obedient Servant,

S. N.

In an account of the Feroe islands, written by a Danish clergyman, a translation of which is given in Dr. Aikin's elegant work the Athenæum, I find the following passage. "The imber, or ember goose, colymbus immer, is one of the most beautiful birds in Feroe. It is as large as a common goose, and lives constantly on the dry land; and although it has been often seen with grown up young, no person has ever yet found its nest. As it has a large hole under each wing, many have imagined, that it there hatches its eggs. The change in regard to the beautiful ring around the neck observed in some of these birds is in all probability a consequence of their different ages." the imber goose.

This large hole under each wing I do not find mentioned by any ornithologist I have at hand; I should be glad to Peculiarity mentioned.

know

know therefore whether it really exist; and, if it exist, for what purpose it is intended. I can scarcely suppose it to be for the purpose of hatching its eggs, as in this case, I think the bird must have been caught occasionally with an egg in it. Lewin, who studied more from nature than from books, says in his beautiful work, the Birds of Great Britain, that we have no information to be depended upon of the manner of breeding of the divers, which he makes, I conceive with propriety, a distinct genus. He conjectures, however, that they make a floating nest on retired waters like the grebes.

Inconsistencies in other parts of the description.

I would farther remark, that the first and last sentence in the paragraph above quoted seem more applicable to the northern diver, colymbus glacialis, than to the immer: but the assertion in the second sentence, that "it lives constantly on dry land," appears altogether inapplicable to any of the diver genus. Lewin, speaking of this genus says, "if we consider the situation of their legs at the extreme part of the body, and their having scarcely any thighs, we must be convinced, that they were not intended for walking: we cannot suppose therefore, that these birds are inhabitants of the land, but that they breed and live on the water only." In fact, if nature webbed their feet for them to live on dry land, she has done something in vain.

XI.

On the Gold Mines in the Department of the Isère, by HERICART DE THURY, *Mine Engineer**.

French gold mines known to the old writers.

THE existence of gold mines in France has long been questioned, though old writers have given us the most positive accounts of them; and the auriferous sands have been considered as our only sources of this metal. The want of accuracy in terms, uncertainty of place, vague information, and mysterious air of the mountaineers, who are naturally mistrustful, have long prevented credit from being given to

* Abridged from the Journal des Mines, vol. XX, p. 101.

the

the repeated stories of discoveries of gold mines: but when the intendants, by order of the regent duke of Orleans, caused a general search to be made into all the metallic and mineral matters throughout the kingdom; when collections of these substances were formed, and they were carefully described; and when accurate analyses had been made by enlightened chemists; the wealth concealed in the French territories could no longer be questioned. Dauphiny was then cited as one of our richest provinces; its mines attracted the attention of government; information considered till that time as vague and uncertain was collected with care; skilful men were employed to examine these indications, and it was soon found, that Dauphiny really possessed several gold mines, some of which appear to have been known and worked in very remote times.

Dauphiny one of the richest provinces in mines.

The mines are of two different kinds: some affording native gold; others containing this metal mixed or so intimately combined with different metallic substances, that its presence is to be detected only by the assay.

Two kinds of them.

The native gold mines of what was formerly the province of Dauphiny are, 1, that of la Gardette; 2, that of Dormillouse or la Freissinière; 3, those of Orel; and 4, the auriferous sands of the Rhone. The mine of Dormillouse is in the present department of the High Alps, and that of Orel in the department of the Drôme.

Native gold mines.

The mountain of la Gardette rises above the village of the same name, in the commune of Villard-Eymont, near four miles south of the town of Oisans, and about six from Allemont. Its mine was included in the circle of mines granted to Stanislaus count of Provence, brother of Lewis XVI, by a decree of the council of state in 1776.

Mountain of la Gardette.

Mines granted to the count of Provence.

This mountain, which is 1290 met. [1410 yards] above the sea, and 550 m. [600 yards] above the town of Oisans, has at its foot a perpendicular cliff above 200 m. [218 yds.] high. Its base is a reddish granite, composed of red feldspar, green steatitic quartz; and gray mica. Above this is a laminar quartz rock of a blackish gray, the strata of which run S S E and N N W, and have an inclination of 33°.

Geology of the mountain.

This micaceous rock, in which the vein of gold is found, is covered by a secondary limestone, which forms the whole of

Gold vein.

of the upper part of the mountain. This is of a deep blue gray, and contains belemnites and ammonites. The inclination and direction of its strata vary greatly, but in general they incline to the north at a greater or less angle, which appears to be determined by the slope of the primitive rock, on which this limestone rests. At the southern extremity of the mountain, below Villard-Eymont, the micaceous rock is covered by amygdaloid hornstone, which from the decomposition of its calcareous nodules assumes a pseudovolcanic appearance.

The vein of la Gardette is quartz in mass, crystallized wherever the siliceous matter has not been sufficient to fill the whole of the vein. It is enchased in gneiss. Its direction is W N W; its dip to the south, 80°; its thickness varies from 60 to 80 or 90 centim. [1 ft. 11 inch. to 2 ft. 11 inch.] and upwards. Its length has been ascertained for more than 450 met. [492 yrds.] from the foot to the summit of the mountain.

Attempts to work it.

The first attempts to work it were made in the beginning of the last century by some of the mountaineers, who gave it up for want of money or of skill. In 1733 some researches were made by order of the king, but they were badly conducted, and without success. In 1765 some of the inhabitants of Gardette attacked it anew, to obtain rock crystal. Their labours were confined to a shaft of 11 met. [12 yds.] deep, in which they found some indications of gold, in crystals of sulphuret of lead deposited on needles of quartz. In 1770, after the discovery of the silver mine of Chalanches, one Lawrence Garden tried the vein, and after several days labour he found in the gangue many specimens of gold distinctly marked. These he carried to the foundry of Allemont. Mr. Binelli, its superintendant, assayed them, ascertained the presence of the gold, and paid a visit to the spot; but he could not be persuaded, that the specimens he had assayed were taken from this vein. In 1779 however, Mr. Binelli being succeeded by Mr. Schreiber, Garden carried some specimens to this gentleman, who visited the place, and found, that the vein really contained gold. On the report of Mr. Schreiber, the count of Provence ordered the mine to be explored, and the works were begun in June

June 1781, and they were continued with much diligence till 1788.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of this Society on the 11th of February, Professor Jameson read a short account of the oryctognostic characters and geognostic relations of the mineral named cryolite, from West Greenland. Cryolite.

Mr. P. Neill read a description of a rare species of whale, lately stranded near Alloa, in the Frith of Forth. It measured 43 feet in length; had a small dorsal fin; longitudinal sulci in the thorax; short whalebones *(fanons)* in the upper jaw; the under jaw somewhat wider, and a very little longer than the upper; both jaws accuminated, (at least, considering the bulk of the animal, they might be so described), the under one ending in a sharp long ridge. From these characters he considered it as evident, that it was the *baleinoptera acuto-rostrata* of la Cepede, and that that author had fallen into an errour in saying, that this species never exceeds from 26 to 29 feet long. Rare species of whale.

At the same meeting, the Secretary laid before the Society several interesting communications. 1. Copies of the affidavits made before the Justices of the Peace at Kirkwall in Orkney, by several persons who saw and examined the great sea snake *(halsydrus Pontoppidani)*, cast ashore in Stronsa in October last; with remarks illustrative of the meaning of some passages in these affidavits.—2. An account of the discovery of a living animal resembling a toad, inclosed in a bed of clay, in a cavity suited to its size, at the depth of 57 fathoms, in the coal formation at Govan; communicated by Mr. Dixon, of Govan Hill.—3. An instance of remarkable intrepidity displayed by an old male and female otter, in defending their young, although the otter Great sea snake. Toad in a bed of clay. Intrepidity of an otter.

otter is generally accounted a very timid animal; communicated by Mr. Laskey, of Crediton.

Collection of British shells.

At this meeting also, Mr. Laskey (who is at present with his regiment at Scotland, and who is well known in the scientific world as an eminent conchologist), presented to the Society a very valuable and well arranged collection of British shells, and likewise a curious mineral from New Holland.

Royal Society.

Royal Society.

IN the Bakerian Lecture read in December last, before the Royal Society, Mr. Davy has given an account of various experiments on ammonia, sulphur, phosphorus, charcoal, the diamond, plumbago, and the fluoric, boracic, and muriatic acids. The results seem to prove,

Ammonia contains oxigen.

1. That ammonia contains oxigen, which he had ventured to infer from facts detailed in his Bakerian Lecture for 1807; and that in the action of ammonia and potassium, it is the ammonia that is decomposed, and that there is not the slightest evidence in favour of the existence of hidrogen in the metal of potash.

No hidrogen in potash.

Sulphur and phosphorus.

2. That sulphur and phosphorus contain hidrogen and oxigen, and that they are probably combinations of these matters with bases, which have never yet been obtained pure; and that they are analogous in their constitution to the oily bodies, except that these last have for their bases the carbonaceous element.

Carbon.

3. That charcoal in its purest form contains hidrogen; and the diamond probably a small quantity of oxigen; and that the purest form known of the carbonaceous element seems to be in plumbago, where it is alloyed with iron.

Fluoric acid.

4. That the fluoric acid may be decomposed and recomposed, and that its basis is analogous to the sulphureous basis.

Boracic acid.

5. That the boracic acid is likewise susceptible of decomposition and recomposition; and that it furnishes a peculiar basis, which is more analogous to charcoal than to any other species of matter.

Muriatic acid

6. That muriatic acid gas in its common form contains at least one third of its weight of water; and that, when the

the substance is obtained free from water, it is more conducting as to elecricity, does not redden vegetable blues, and in different combinations detonates violently with potassium. Whether the muriatic basis be separated in these explosions, Mr. Davy has not yet been able to ascertain.

Nitrogen decomposed.

In a communication read before the Royal Society, February the 3d, Mr. Davy has detailed various experiments on the distillation of substances formed by the action of potassium on ammonia, which he conceives cannot be explained on any other supposition, except that nitrogen is a compound of oxigen and hidrogen, or that ammonia and water consist of the same kind of *ponderable* elementary matter. Whichever conclusion may be finally adopted, the decomposition of nitrogen in the process is sufficiently evident.

Correction of an errour.

By an errour of the press in the extract from Mr. Davy's letter in our last number, 1000 is put for 100. The context sufficiently shows this mistake; but for greater security I notice in this place, that 100 plates are quite sufficient for decomposing potash.

TO CORRESPONDENTS.

Dr. ROBERT HARRINGTON, of Carlisle, must allow me to decline any engagement as to publishing his intended communication, till after it may have been submitted to my perusal. I must also beg leave to say, that neither the threat nor the insinuations contained in the letter, which conveys the proposal I have rejected, are, in my apprehension, entitled to notice.

I am much obliged by the communication of Mr. Kemp, which I am prevented from making use of, because his researches have been anticipated; as may be seen in the 2d edition of my Chemical Dictionary, and in Rees's Cyclopedia, Art. Copal.

METEOROLOGICAL JOURNAL

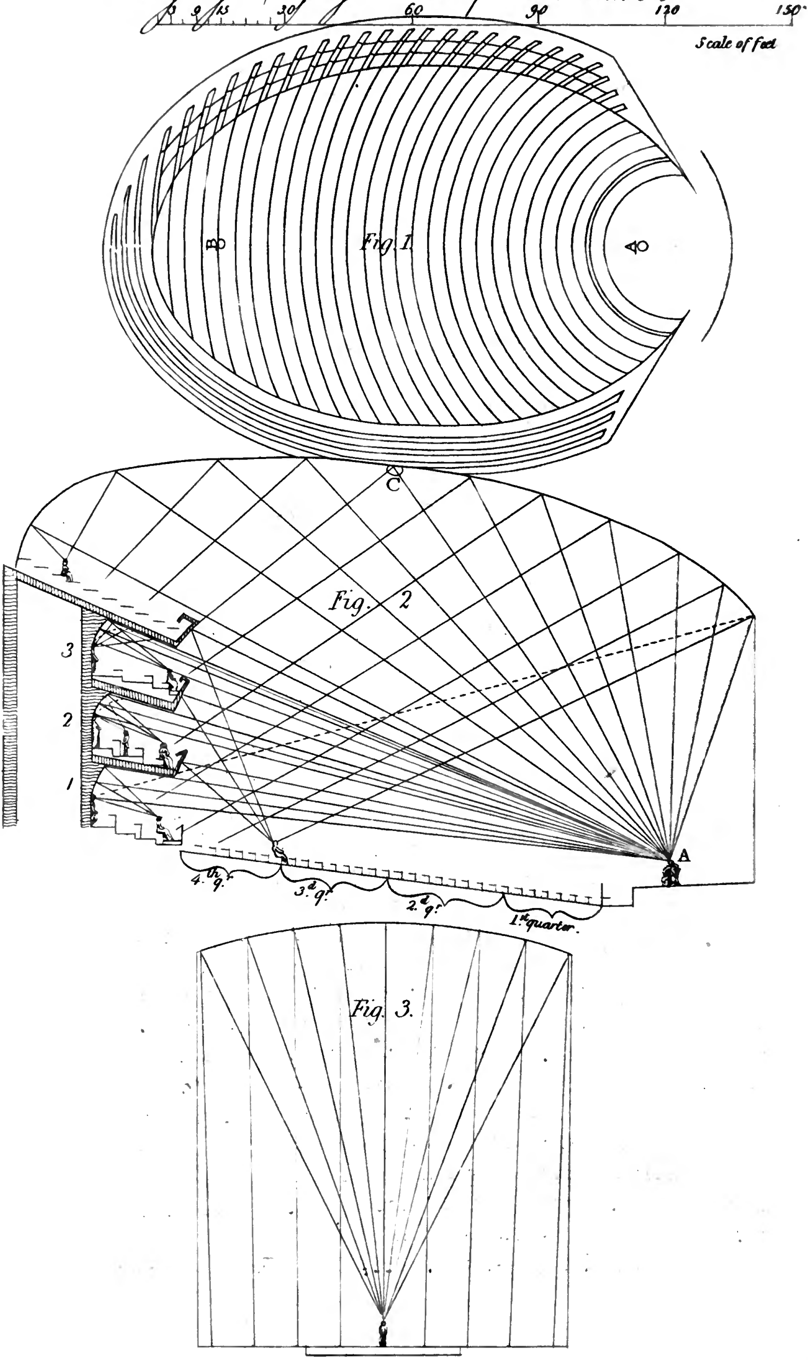
For FEBRUARY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JAN. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
27	48	48	46	46	29·51	Cloudy	Rain
28	48	48	50	46	29·59	Ditto	Fair
29	46	47	48	45	29·26	Fine	Rain
30	45	46	48	43	29·32	Rain*	Ditto
31	42	42	44	40	29·76	Cloudy	Fair
FEB.							
1	46	50	52	48	29·77	Ditto	Ditto
2	48	49	51	47	29·51	Rain	Rain
3	50	45	52	42	29·21	Fair †	Ditto
4	44	43	46	42	29·45	Ditto	Ditto
5	43	43	46	42	29·38	Ditto	Ditto
6	44	46	48	36	29·42	Rain ‡	Ditto
7	38	35	40	31	29·89	Cloudy	Ditto
8	32	33	34	33	29·90	Rain & hail	Cloudy
9	42	48	50	44	29·39	Ditto	Rain
10	47	47	50	44	29·28	Cloudy	Ditto
11	44	46	48	41	29·21	Ditto	Ditto
12	44	48	49	40	28·83	Ditto	Ditto
13	44	46	49	42	28·81	Ditto	Ditto
14	45	47	49	42	29·26	Ditto	Ditto
15	46	46	51	41	29·70	Rain	Ditto
16	44	47	49	45	29·83	Ditto	Fair
17	46	49	53	44	29·58	Cloudy	Ditto
18	48	40	55	38	29·80	Fair	Rain
19	42	46	48	44	29·46	Cloudy §	Fair
20	46	40	50	34	29·27	Fair	Cloudy
21	38	33	42	32	29·97	Ditto	Fair
22	34	38	46	33	30·33	Ditto	Ditto
23	44	42	50	39	30·10	Ditto	Ditto
24	42	42	46	38‖	30·31	Ditto	Ditto

* Rain, tremendous wind.
† At 11 Orion and the Moon brilliant.
‡ At 11 fine, inclining to frost.
§ Fine till 8. Halo circumscribing the Moon, indicating moisture.
‖ During the conflagration of Drury Lane Theatre, a thermometer hanging opposite it in my Observatory rose 5 degrees. The distance is near 600 yards.

Sir George Cayley's Plan for a Theatre.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1809.

ARTICLE I.

Plan for an improved Theatre: by SIR GEORGE CAYLEY, *Bart. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR, *Brompton Malton, Yorkshire, March* 1, 1809.

IN consequence of this second theatrical disaster within so short a period, I take the liberty of enclosing, for the use of your valuable Journal (if you think proper), a plan, and letter of explanation respecting it, which I submitted to the proprietors of Covent Garden Theatre some time ago, but which I am just informed by Mr. Kemble has not been able to be adopted in any degree by Mr. Smirke, their architect. If you think the paper worth printing, or any extracts from it (which might be more proper) it is much at your service; if not, you will have the goodness to return it. As a new theatre will soon have to be built, and as the public is so much interested in having it as convenient as possible, publishing a few hints of this kind, however imperfect, may call forth others of more value; and thus at length some good may arise.

Plan for a theatre.

Imperfect hints may be improved upon.

Theatres are objects of importance.

The ancients bestowed great pains upon their theatres, and they are certainly an object of considerable importance in society. I conceive there can be no difficulty in blending the beautiful with the useful in the construction of these buildings, if they do not indeed go so much hand in hand, as almost to be inseparable. You will perceive, that the plan I allude to is constructed upon the principle of applying all the sound, that can be caught by the reflecting surfaces of the building, to those parts only of the theatre where the direct voice of the actor may prove too weak to be heard distinctly. There should be a note inserted to this effect in some part of the letter, *viz.* that as no similar sounds uttered in succession to each other can be heard separately by the finest ear, when the interval of time between each does not exceed $\frac{1}{10}$ of a second; therefore, no reflected portion of a sound that arrives at the ear within less than $\frac{1}{10}$ of a second after the direct pulse will make a separate impression, or echo, but will add to the strength of the former impression. Hence, as sound travels about 1142 feet per second, unless the difference between the whole distance travelled over by the reflected pulse and that of the direct pulse be greater than 114 feet, no echo will be perceived. In the plan of the theatre sent, the greatest difference of this sort only amounts to 68 feet; therefore, even in this instance, about one half of the influence of the reflected pulse would be exerted in adding to the intensity of the sensation of the direct pulse, and about one half in prolonging a nearly similar impression.

Convenience should be united with beauty in them.

The principle here to strengthen the voice by reflected sound.

No echo would be heard within certain limits.

I must apologize for writing to you in so slovenly a manner: I am much hurried, being just setting off on a journey, and I conceive, that, unless these hints can appear in your next number, they will be forgotten before another theatre requires planning. In the mean time I beg to thank you for much pleasure and information conveyed to me by your Journal.

I have the honour to remain, Sir,

Your obedient servant,

GEO. CAYLEY.

To

To JOHN KEMBLE, Esq.

SIR, *Brompton, Sept.* 25, 1808.

Theatres should be contrived for convenience of seeing & hearing.

SINCE the lamentable accident, that has so lately happened to the Covent Garden Theatre, the frequent occurrence of that event to my thoughts has led me to speculate upon the various improvements, that might be made in the construction of theatres. I have taken the liberty of enclosing you the following plan and hints, which I conceive to be worthy your attention, in as much as they state undoubted principles, which local convenience may more or less permit to be put in practice, but without an attention to which no theatre can be pronounced well constructed. The science of acoustics is perfectly well understood, and the enclosed rough sketch of the internal plan and elevation of a theatre is modified to the principles of that science, in conjunction with giving the greatest possible convenience of sight to the largest number of people the space can contain.

Advantages of the elliptical form.

It is the property of an elliptical room, to collect all the sound uttered in one of its foci into the opposite focus by reflection; hence, as the ellipsis is a very beautiful curve, and as it is only the parts of a theatre distant from the stage, that require the aid of reflected sound, I have adopted this figure, as the ground plan, Pl. VIII, fig. 1, will show. Here any voice uttered upon the stage at A would be concentrated at the point B, excepting what is absorbed by entering the side boxes.

Semicircular stage.

Advantages & disadvantages of seats concentrical with it in the boxes.

I have drawn the stage semicircular, and on one side arranged the seats concentrical with it. This, I conceive, would be a material benefit to the observers, but it would have this objection, *viz.* that the seats, if so placed, must rise in steps and have arms to each; hence the necessary allowance of room for the accommodation of the largest persons would be more than necessary for smaller ones; and on no occasion, however pressing, could the advantage be taken of sitting closer.

Scenes should be segments of circles.

I have also drawn the scenery in a portion of a circle, which would be a most material advantage, both to the hearing and sight, *if conveniently practicable:* and provided double the height of a scene can be had within the building

ing, it might be managed by suspending the scenes on cords passing over rollers disposed in this form.

Economy of space.

In constructing the elevation of a theatre, the first consideration is to economise space, hence in the boxes, as at No. 1, fig. 2, after allowing the seats to rise one foot in five for the purpose of clearing the view from the heads of those below, if a line be drawn to the top of the scenery from the eye of the most backward observer, the bottom of the next tier of boxes must just commence at that line, as exhibited by dots.

Theatres too large for the voice to fill with ease,

As it is advantageous in the Metropolis to make theatres more extensive than the direct voice of an actor can fill with ease, it becomes necessary, to call in the aid of reflected sound, and so to distribute the whole voice as may be deemed most important. I have in the enclosed sketch supposed, that (in a theatre where the extreme part of the pit is 120 feet from the centre of the stage) the direct voice is sufficient till within one fourth of the extremity of the building. Therefore, the roof is so curved, as to commence its reflection at that point, as may be traced by following the progress of the pulses of sound emitted by the actor at A. One half the roof, as far as C, is allowed to give the sound it receives over this portion of the pit, and the three tiers of boxes. The remaining half of the roof is employed in throwing its sound upon the upper gallery, increasing the density of its reflection as the distance from the stage increases. Although this gallery receives the influence of half the ceiling, yet from the oblique position of it, it will not catch more than half as much sound as the other portion, which is fully required by the distance of the hind part of the gallery, the direct sound being there 25 times less dense than in the quarter of the pit next the stage; whereas by the reflection this disproportion will be reduced to about ten times only, and of course it will be as distinctly heard as in the third quarter of the pit.

this therefore should be aided by reflected sound.

Boxes.

The ratio of sound in the three front boxes compared with that of the first quarter of the pit, is as $\frac{1}{16}$ to one; this, by the reflection of half the roof will be reduced to about $\frac{1}{7}$, hence these parts will hear nearly as well as the centre of the pit. In addition to this, the back of each tier of boxes should be covered so as to give a focus of sound

sound either to the front, middle, or last benches, as thought best, this shown at 1, 2, 3. The two latter, where altered from the former, by dotted coves. The fronts to the boxes should present reflecting curves, to throw their sound within the fourth region of the pit.

Fig. 3 is a hind view, showing the proper curve of the roof in this position, where the only object is to keep the diverging rays of sound parallel after reflection, and clear of the sides of the boxes.

I think it would not be particularly expensive to have the whole beam and pillar work of the theatre of cast iron; and likewise to make the elliptical part capable of being completely cut off from any fire in the other part of the building, by a jointed sheet iron curtain, to close up the stage every night after performance, or in case of fire, during the play hours. The name of this would be very attractive. Cast iron beams and pillars.

To prevent the bad effect of squeezing from sudden alarms, no door about a theatre should open inwards, and the outlets should be as large as possible, and some extra ones easily opened if necessary: a good reservoir of water, and an engine or two on the spot, are precautions too obvious to need an observation. Doors to open outwards.

I have the honour to remain, Sir,

Your obedient servant,

GEO. CAYLEY.

II.

A Letter on the Alterations, that have taken place in the Structure of Rocks, on the Surface of the Basaltic Country in the Counties of Derry and Antrim. Addressed to HUMPHRY DAVY, *Esq., Sec. R. S. by* WILLIAM RICHARDSON, *D. D.*

(Concluded from p. 175.)

Inquiry into the Formation of our perpendicular Façades.

IT is natural, that the first great operation we proceed to investigate should be the formation of our magnificent façades, Formation of the cliffs.

çades. one of which is the principal subject of this memoir.

The line of coast, that bounds our basaltic area on its north side, extends about twenty-five Irish miles, in which course the precipices are nearly continuous, and more than one half of them absolutely perpendicular for a great part of their stupendous height. The operation by which they were cut off so abruptly, and left with a formidable aspect towering over our coast, is the one we inquire into.

They once projected farther.

That these bold precipices once projected farther in many places is easily demonstrated; at *Beanyn Daana*, and at the *Chimney*, the columnar construction was obviously once carried much farther out.

At the *Milestone*, *Portcooan*, and *Portnabau*, the fragments of dikes extend far beyond the face of the precipice.

Not formed by the sinking of a part, or any violent convulsion,

These same facts, together with the projecting base, show, that these sudden abruptions were not formed by the subsiding and sinking of one part, leaving the remainder in its place: still less by any violent revolution, or convulsion; as the stratification has not sustained the slightest shock either above or below the façade.

or by the beating of the waves.

The formation of our abrupt coast has been ascribed to the action of the sea beating violently against it, washing away the lower parts, and leaving a perpendicular façade standing; as we often see on the banks of rapid and encroaching rivers.

For the cliffs are too high,

A cool examination of our precipices will soon prove, that our facades could not have been so formed, for we always find them in the highest part of the cliff, and receding from the water, which could be instrumental in bringing down the materials from above only by washing, and so wearing away the bases of the steepest parts; but the elevations of these bases are utterly irreconcilable to this supposition; for instance, the base of *Pleskin* façade is two hundred feet above the present level of the sea, that of *Fairhead* three hundred; now had the sea ever risen to either height, it would have submerged a great part of *Ireland*, and none of the neighbouring country (whatever its level may be) bears the least resemblance to alluvial ground, nor shows any mark of having been once covered by the sea.

The

The next argument is still more conclusive; the boundary of our basaltic area on its north side is for twenty-five miles also the confine of sea and land; so far it is natural to ascribe its features and characteristic marks to the action of the powerful element that beats against it. But when that precipitous boundary ceases to be the confine of sea and land, turns southward towards the interior, and becomes the line of demarcation between the basaltic and schistose country on the west, it still preserves its former character; that is, of a range or ridge of very high land, steep to the exterior, and sometimes cut down vertically into façades, like its northern part that lines the shore. and occur eqnally inland.

Thus *Magilligan Rock*, (four miles inland) is not inferior in magnificence to any of our façades on the coast, its perpendicular section is one hundred and seventy feet, and this continuous for a mile; the façades at *Bienbraddock* are nine miles farther inland, and those of *Monyneeny* thirteen; while the base of the lowest of these perpendicular precipices is elevated 1400 feet above the sea. Instances.

The same style prevails on the east side of our basaltic area, after its boundary ceases to be the confine of sea and land; for the limestone façades at *Garron Point*, (considerably above the level of the sea) exactly resemble those of *Dunluce* and *Kenbaan* at the water edge; and *Cave Hill* (several miles from the sea, and nearly one from the shallow estuary of *Belfast*,) exhibits basaltic façades at the height of one thousand feet, precisely similar, and little inferior to those of *Magilligan*.

The exact resemblance between our inland façades (on the east and west sides of our area) to those on the shore, proves them to be all effects from the same cause, and that our accumulated strata have in all these similar instances been cut down vertically by the same agent, and that this agent was not the sea. These resemble those on the coast.

Nor has this powerful agent confined its operations to our coast, or to the periphery of our basaltic area; we can trace it over its whole surface; we find throughout its interior, similar, though very diminutive abruptions, executed precisely in the same manner, that is, strata cut across by a long vertical façade, their planes on the upper side perfectly undisturbed, Similar appearances in the interior of the basaltic country.

undisturbed, while on the lower side all the materials of which that part of the stratum was once composed are completely carried off.—(See 6th fact.)

We are now unavoidably led into a discussion of a question, which has at all times occupied the attention of naturalists.

Whence arise the Inequalities, with which the Surface of the Earth is so exceedingly diversified?

Whence arise the inequalities on the surface of the Earth?

I shall not attempt to encounter this question generally, nor to extend my inquiries beyond the limits I have prescribed to myself; but I shall try whether the curious facts, so profusely exhibited over our basaltic area, throw any light upon the formation of our own inequalities. or lead us a step toward the discovery of the operations, by which such stupendous effects have been produced.

Some say from original formation,

Some, to escape the difficulties in which this question is involved, ascribe our inequalities to original formation; as if the world had come from the hand of the Creator with the variegated surface, which now contributes so much to its beauty. But the frequent interruptions and resumptions of the strata in our area, with the perfect resemblance of the corresponding parts, however great the interval by which they are separated, can scarcely leave a doubt, that these strata were at first continuous; of course the figure of our surface at that time must have depended on the original positions and inclinations of these strata, which, as appears by the 3d fact, are now unconnected with the superficial line, the figure of which is governed by their abruptions and removals alone.

others from causes within or on the surface of the Earth.

Naturalists have differed much in opinion as to the direction, in which the causes acted that produced the inequalities on the surface of our globe; some referring us to the bowels of the Earth as to the scene of action; while others assert, that the operations were performed upon the surface itself.

But the slightest inspection of our façades will at once prove, that the first hypothesis cannot be correct; for obliquity of direction must have been the result of a disturbing cause acting from below; whereas parallelism and a steady rectilineal

rectilineal course distinguish the basaltic arrangements of which I have been treating.

We have, it is true, occasional depressions of our strata, where they obviously have subsided, and no doubt from a failure of support below; but in no instance that I have met with, in our area, are these attended by the slightest concussion; the permanent and subsided parts, with us, still preserve their parallelism, and the continuity of their material; whence it is probable this event took place previous to the induration of the strata, and of course antecedent to the period, to which I limit myself.

Sometimes strata have subsided from failure of their supports.

Buffon ascribes our superficial inequalities to the agitation of the waters while they covered our Earth, and argues from the resemblance these inequalities bear to the waves of the sea; a resemblance I cannot trace in any country, which I have observed; nor could any sudden and perpendicular abruptions ever have been produced by any agitation of the waters.

Buffon's hypothesis.

Professor Playfair considers rivers as having formed not only the beds, or channels in which they flow, but also the whole of the vallies through which they run, and in general all the inequalities of our surface; but an attentive observer, tracing the course of any of our most rapid rivers, would soon perceive, that the quantity of its depredations have been comparatively insignificant, and that they can be determined with precision; the river has no doubt in several places extended itself considerably on both sides; but in the intermediate space, between the remotest boundaries it ever reached, it levels, instead of raising inequalities.

Playfair's.

The same result I apprehend would follow from the operations of another agent, which theorists are in the habit of calling in to their aid, when they cannot find some certain material, which from their theory we have reason to expect; they then tell us it has been carried off, and lost in the suite of degradations and decompositions.

Decay and decomposition.

But delay and decomposition, instead of creating inequalities, would produce a contrary effect, and deface those actually existing; they would gradually abate the height of our perpendicular façades, and increase the green steep at their bases by the accumulation of the crumbling and mouldering

But they would wear down the prominences.

mouldering materials from above; while the more diminutive façades, formed by the abruptions of single strata scattered over the face of our area, and forming its most characteristic feature, would in time (as many are already) be converted into steep acclivities covered with verdure.

The effects first to be examined.

Such are the principal causes, to which the inequalities of our surface have been generally ascribed. Previous to our deciding finally upon their insufficiency, it may be proper to enumerate a few of those inequalitities, where the deviation of our present surface, from the form it probably had originally, is not only striking, but where also the concomitant circumstances afford demonstration, that some great operation has once taken place there.

Thus, by making ourselves acquainted with effects, we shall be better qualified to investigate causes; and if these effects shall appear to be beyond the powers of such natural agents as we are already acquainted with, we shall be justified in admitting the performance of operations, to which we have seen nothing similar; and also in admitting the former existence of powers of far superior energy to any we have ever known in action.

Enumeration of some remarkable Inequalities in the Surface of our basaltic Area, produced since the Consolidation of its Strata.

Remarkable inequalities produced since the consolidation of the strata.

That we may better understand the facts I am proceeding to state, I shall assume (in the style of the mathematicians *puta factum*) previous to demonstration, that the planes of our uniform, rectilineal strata, however interrupted we may now find them, were once continuous.

Upon this supposition, the valley of the *Mayola*, between the stratified summits of *Seafin* and *Slievegallon*, is an excavation 1700 feet deep, and three miles wide, of which the whole materials have been completely carried off.

To the northward of this excavation, between *Seafin* and *Carntogher*, the continuous accumulated strata of basalt are interrupted, and taken away quite down to the schistose substratum; while the untouched summits of the contiguous mountains, *Carntogher*, *Seafin*, and *Monyneeny*, are still stratified basalt.

On

On the eastern side of our area, immediately to the southward of *Kello* and *Connor*, a similar operation has been performed, attended by still more extraordinary circumstances. Remarkable inequalities produced since the consolidation of the strata.

We here find a district near four miles in diameter, called the *Sandy Braes*; over this whole space the basaltic stratification has been carried off, and the operation has reached deep into a very singular substratum; a reddish breccia, by some mineralogists called a porphyry, the mass friable, but the component angular particles of extreme hardness.

The hills, of which this little district is full, are every one perfect segments of spheres; while the loftier basaltic hills that surround it preserve their characteristic form, to wit, a gradual acclivity on one side, with a steep abruption on the other.

As we sail along our northern shore we discover another great chasm or interruption of our strata, which also cuts deep into the substrata: for on the west side of *Ballycastle* pier, the bold basaltic precipices suddenly disappear, and at a lower level disclose the substratum, which appears to be an alternation of sand-stone and coal, sometimes with bituminous schistus.

A mile or two to the eastward the abrupt precipice is resumed, and a basaltic stratum again occupies its summit on to *Fairhead*, with the same angle of inclination in which it was disposed along our whole coast, that is a slight ascent to the north.

Traces of similar operations and abruptions are to be found over our whole area, but the preceding are sufficient to make us acquainted with the style of these interruptions of our strata; of course it is time to proceed to the suspended demonstration, that our strata, so interrupted, were once continuous, notwithstanding the magnitude of the interval by which the corresponding parts are now separated.

Proofs that our now interrupted Strata were once continuous.

We must now turn back to the façades of *Bengore*, where the strata themselves, and all the circumstances attendiag them, are so happily displayed, as to throw great light Proofs that the interrupted strata were

once continuous.

light on the subject, and to lead us analogically, step by step, to the conclusion we seek for.

Let us examine and trace the summit of the precipice for a mile immediately eastward from the *Giant's Causeway*, and we shall find a frequent interruption and resumption of the fourth, fifth, and sixth strata, at the shortest intervals, the interruption not always reaching to the lowest of the three, which in that case remains continuous: so far simple inspection removes all doubt, that each of these strata was once contiguous as far as the great depression to the west of *Pleskin*.

Here indeed the interruption becomes considerable, not less than a mile; but when we find at *Portmoon* a succession of three strata with the same inclination, in the same order, of the same thickness each, and with the same strong characteristic marks, that distinguished the three interrupted, at the depression; above all, when we find the strata they rest upon continuous (at least with very trifling interruptions) for the same extent; I think we can scarcely entertain a doubt, that this interval between the corresponding parts, though so much greater than any of the preceding, is, like them, but an interruption, and that these strata were once continuous from the depression to *Portmoon*.

The same style of induction would establish the quondam continuity of all the strata in the face of *Bengore* promontory, for here the strata are so distinctly marked, that we know each of them, when we find it again after any interruption.

In the rest of our precipices and façades, the similarity of the strata deprives us of this advantage; yet in their smaller interruptions, the eye, by tracing the rectilineal course of the strata, and so connecting the separated parts, can establish their former continuity: while in the greater intervals we must rest our proof on analogy alone.

That we may be entitled to carry this style of induction into the interior of our basaltic area, and to apply the same reasoning to enable us to form a similar decision upon the more stupendous interruptions of our strata, which I have already enumerated, it becomes necessary to explain the geological

geological construction of our area,—the strata of which it is formed—the arragement—and their inclinations.

Proofs that the interrupted strata were once continuous.

An extensive limestone stratum, white as chalk, and about two hundred feet thick, seems to form the base of the whole district I limit myself to: upon this accumulations of rectilineal and parallel basaltic strata are heaped up to most unequal heights.

This great calcareous stratum seems not to be an accurate plane, but rather to resemble a basin, as every where at its periphery it dips to the interior; yet the plane of its section has a slight ascent to the southward. A recollection of these circumstances will enable us to account for every appearance this stratum exhibits, as it happens to be disclosed to us; and by the converse, an attention to these appearances will enable us accurately to determine the position of the stratum.

This stratum, from *Ballycastle* to *Solomon's Porch*, (about twenty-five miles,) keeps very nearly the level of the sea, often indeed sinking below the surface, but never raising its lower edge above it; but when, at *Solomon's Porch*, the boundary of our basaltic area begins to deflect to the south-west, and then to the south, the ascent of the stratum to the southward begins to operate, and we perceive the dotted line of its quarries gradually to rise along the face of the mountain from the shore to *Monyneeny* and *Seafin*, where it has attained the height of 1500 feet. It is true, the actual stratum has not been opened at these two great elevations, but the white rubble immediately below the basaltic façade proves incontestibly, that it is close at hand.

An accumulation of basaltic strata had in this southern course, as well as on the northern shore, covered the limestone up to the summits of the hills or mountains.

I have already stated how the ridge of mountain is suddenly interrupted by the valley of the *Mayola*, from 1600 to 1700 feet deep; but if we look to the southward, in the rectilineal course of the strata (the positions of which we have been able to ascertain with so much accuracy), we shall find near the summit of the mountain *Slievegallon* a similar white limestone stratum crowned with basalt, cutting it in the very direction the former ought to have reached

it,

Proofs that the interrupted strata were once continuous.

it, that is perhaps two hundred feet higher; the ascent of the strata to the southward having elevated their planes so much in a distance of four miles, the probable interval between the summits of these mountains.

We are now to decide whether this calcareous and basaltic fragment, on the summit of *Slievegallon* mountain, be the last remnant of the old arrangement we have been tracing, and ascertaining with so much precision, for seventeen or eighteen miles from the sea, and twenty-five miles along the coast; but now interrupted by the valley of the *Mayola*, like our former more diminutive interruptions, and also like them resumed at the next elevation, in the same rectilineal course, the strata preserving the same order, and the same characteristic marks. Or whether these strata, appearing on the summit of *Slievegallon*, be the commencement of a new arrangement, abandoned by nature as soon as begun: which is highly improbable, for neither limestone nor basalt is to be found on the mountain, except in this solitary hummock.

We might, by a minute attention to the inclinations and arrangements of the strata contiguous to the other interruptions I have enumerated, prove in like manner, that the basaltic masses crowning the summits of the surrounding hills and mountains are merely the remnants of strata once extensive and continuous, but interrupted and carried off, as in the preceding case, by the same powerful agent.

The more diminutive inequalities scattered over the whole surface of our area, and always produced by interruptions of the strata, would still more easily admit the application of the same reasoning, from the contiguity of their abrupted parts; but the detail would be tedious; those who wish to pursue the subject farther must come to the scene themselves.

Materials completely carried off.

Materials completely carried off.

A circumstance perhaps still more extraordinary is the complete removal of all the materials, that once filled up the intervals between the abrupted parts of these strata. I have stated in my 9th fact, that the materials, that had formerly composed the projecting parts of our northern façades and precipices, had totally disappeared.

The

The removed parts of the limestone stratum on the west side of our area have shared the same fate: for where the chain of mountains extending from *Magilligan Rock* to *Blenbraddock* is interrupted by the vallies at *Stradreagh*, *Drumrommer*, and *Ballyness*, it is obvious that the limestone stratum was once continuous to the high points where it shows itself on *Keady*, and the mountains on each side; its thickness too, wherever we can try it, is very great; yet this stratum, which in its entire state must have spread like a roof far above the present surface of these vallies (which are now sunk deep into the schistose substratum) has not left a particle of its *debris* behind, nor is a single lump of white limestone to be found, until we come to the quarries, that is, to the edge of the solid, untouched stratum.

Conclusions.

The conclusions that unavoidably follow, from the consideration of these facts, are, General inferences.

That the hills and mountains, in the district I have been describing, were not raised up or formed as they now stand, but that they are the undisturbed remains of strata that were left behind, when stupendous operations carried away the parts that were once contiguous to them.

That the inequalities of this surface were all produced by causes acting from above, and carrying off whatever they touched, without in the least disturbing what was left behind.

Additional Evidences. Basaltic Hummocks.*

The arguments on which I have founded my opinions have hitherto been all taken from the hollows in our surface, and the interruptions in our strata, both which the concomitant circumstances have led me to consider as so many excavations; but the lofty elevations, and the abrupt prominencies rising suddingly from our surface, when minutely examined, lead us irresistibly to the very same conclusion. Additional evidence in the basaltic hummocks.

When you and I examined together the line of our northern façades, we studiously sought for the points where

* Navigators use the word hummock to express circular and elevated mounts, appearing at a distance; I adopt the term from them.

nature

Additional evidence in the basaltic hummocks.

nature had made any change in her materials or their arrangement, hoping that at the junctions of these little systems we should find some facts, that would throw light on the subject; but we generally failed; want of perpendicularity, or other local circumstances, impeding us at the most interesting points.

On the present occasion she has adopted an opposite line of conduct, and in many of the steps she has taken, obtrudes upon us demonstration of what she has done.

Whoever has attended to the exertions of man, when employed in altering our present surface, either by levelling heights, or by making excavations, must have observed that it is the practice of the workmen to leave small, cylindrical portions standing, for the purposes of determining the height of the old surface, and thereby ascertaining the quantity of materials removed.

To these may be compared the stratified basaltic hummocks so profusely scattered over our area, and which seem to show how high our quondam surface once reached.

The hummock of *Dunmull*, three miles south-east from *Portrush*, is very beautiful; it stands on the top of a high ridge, and is a conspicuous object from all parts of the country; it is exactly circular, its flat surface contains an acre, it is completely surrounded by a perpendicular façade about twenty-five feet high, and formed by two strata, a columnar, and an irregular prismatic resting upon it.

From this elevated station, where I had the pleasure of accompanying you, I showed you at six or seven miles distance to the westward, among the *Derry* mountains, the still loftier hummocks of *Altabrian* and *Sconce*, hemispherical in form, composed of but one stratum each, while their swelling-out bases displayed accumulations of many more: and also near the hummock of *Fermayle*, resembling *Dunmull*, but much larger, and also surrounded by a façade composed of two strata.

I showed you also at twenty miles distance to the south-east the gigantic *Slemish*, one of our basaltic hummocks, magnified (as it were) into a lofty and insulated mountain, completely stratified from its base to its flat summit.

I showed you likewise from the bottom of its ridge the neat

neat but diminutive hummock called the *Rock of Clogher*, above *Bushmills*. As our time was precious, you took my word for its stratification being precisely similar to that of *Dunmull*.

Additional evidence in the basaltic hummocks.

There are many other basaltic hummocks scattered over the surface of our area, all of them either stratified or portions of strata; two of the most remarkable are the hill of *Knock Loughran*, near *Maghera*, and a tall hummock (the name of which I forget) a mile eastward from *Lisanoure*.

We meet still more frequently an imperfect style of hummock, a semicircular façade on one side, while on the other it slopes away gradually with the dip of the strata, as if the operation had been interrupted before it was carried quite round; the most remarkable of these are *Ballystrone*, in *Derry*, and *Croaghmore*, in *Antrim*, both visible from *Dunmull*.

Regular stratifications on the summits of hills and mountains have long been a stumbling block to theorists. The historian of the French Academy, for the year 1716, obviously ascribing the superficial inequalities of the Earth (like many others) to causes acting from below, and perceiving how incompatible such assemblages of strata were to his theory, thinks it safer to doubt their existence, as they could not have been formed, he says, " unless the masses of the " mountains were elevated in the direction of an axis per- " pendicular to the horizon: *ce que n'a pu être que très* " *rare*."

But as these stratified mounts are in our area by no means uncommon, they lay us under the necessity of suggesting another alternative similar to those we have already stated.

Were these isolated hummocks originally formed as they now stand, (solitary and separate from each other) one by one? or are they the last remaining portions of a vast consolidated mass, of which the intermediate and connecting strata have been carried off by causes, with which we are unacquainted?

To be able satisfactorily to resolve this alternative, it becomes necessary, to take a careful view of the contiguous countries, and to try whether their construction, and the ar-

 rangement

Additional evidence in the basaltic hummocks.

rangement of their strata, will throw any light upon the subject.

When we examine the assemblage of hummocks above *Knockmull*, that is, *Sconce*, *Fermoyle*, and *Altabrian*, we find their materials and stratification precisely similar to that of the country below them to the eastward, where the abruptions of the strata are displayed in long stony ridges; to the south, the abruptions on the summit of *Keady* mountain discover the same similarity; and to the north-west the grand façade of *Magilligan Rock*, three miles distant, displays an accumulation of exactly the same sort of strata consolidated into an enormous mass.

The hummock of *Dunmull* is formed of two very particular strata, a columnar, and an irregular prismatic; but I showed you, a mile to the northward, at the façades and quarries of *Islamore* and *Craigahuller*, at the base of the hill, that the whole ridge, on the summit of which *Dunmull* is placed, was a consolidated mass, formed by alternate strata of the same description: and that the arrangement of the whole country below, and adjacent, was precisely the same with that of the hummock of *Clogher*, I proved to you at the curious opening of the strata at *Bushmills Bridge*, and in the façades at the *Giant's Causeway*.

After these proofs, that so many (and I might proceed to the rest) of our detached hummocks are in their construction and materials similar to, and connected with, the main consolidated masses of which our country is formed, I think it will scarcely be asserted, that these hummocks were originally formed solitary and separate as they now stand; but rather that they were once parts of that vast whole, and left behind at their present form, upon the removal of the contiguous portions of their strata by some powerful agent, of whose operations and modes of acting we have hitherto obtained little knowledge.

The highest point on the façade of *Cave Hill* is called *M'Art's Castle*, and appears to be a solitary fragment of a stratum, precisely similar to those below it, and obviously once extended like them.

The irregularity of the summit of *Fairhead* plainly shows, that its gigantic columns once reached higher.

And

And in the façade of *Magilligan*, the highest of all, a few desultory patches of an upper stratum (no doubt once perfect and continuous) are to be traced along its summit. Additional evidence in the basaltic hummocks.

Our mountains themselves seem to show clearly, that they were once higher; the top of *Magilligan* mountain is an extensive plain, over which a wandering stratum is interrupted and resumed at intervals for a great way.

At the highest part of *Donald's Hill*, over the valley of *Glenuller*, we find a hummock; also at the termination of the ridge, at its highest part over the valley of *Mayola*, similar hummocks. I have the honour to be, Sir,

Your obedient, humble Servant,

Clonfecle, Jan. 2, 1808. W. RICHARDSON.

III.

Remarks on the Habits of the Imber and Northern Divers, in answer to a Correspondent. In a Letter from THOMAS STEWART TRAILL, *M. D.*

To Mr. NICHOLSON.

SIR,

IN article X of your last number, a correspondent wishes for information concerning the habits of the *Colymbus Immer*. The observation of the Reverend Author of an Account of the Feroe Isles must be incorrect, when he asserts, that "the imber lives constantly on the *dry land*." Author of the account of the Feroe Isles mistaken.

Both the colymbus immer, and c. glacialis are frequently met with among the Orkney and Shetland isles, where I have had many opportunities of observing them swimming about with great velocity, and experienced the difficulty of shooting them, from the celerity with which they dive on the flash, and the very great distance they swim under water. Habits of the imber and northern divers.

The inhabitants of these islands have never seen either of the two species of diver above noticed on the dry land; and, to explain the incubation of a bird which they believe never quits the water, have had recourse to the story of its hatching its egg under its wing, as well as the natives of the Feroe isles. As the egg has never been found in such a situation by any one, it is not easy to discover the origin of such Similar story of its incubation in the Orkneys and Feroe islands.

a story among the inhabitants of the Feroe isles, if they believed that the diver lived constantly on the *dry land*. It seems obviously to have originated among them from a similar cause that produced it among the inhabitants of the Orkney and Shetland islands, a people till lately connected with those of the Feroe isles by frequent intercourse and common language.

This story fabulous.

The truth is, I believe, that the two species of colymbi here noticed live almost constantly on the water; when they leave it, it is only for the purpose of incubation; and they then seem, from the best accounts, always to choose some sequestered spot very near to their favourite element. Indeed the position of the legs of all the divers, and the structure of their feet, show that they are totally unfit to live constantly, or even principally on the dry land. I have never seen either the c. immer or c. glacialis attempt to fly; nor did I ever hear any one say, that they had observed them do so. When closely pursued they swim with great ease and rapidity, and dive for a much longer time than any other bird I recollect to have observed.

They never fly, but swim rapidly, & can stay very long under water.

I am, Dear Sir, yours very truly,

Liverpool, 1809. THOs. STEWART TRAILL.

IV.

Inquiry concerning the Use of Galvanism in Deafness. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Application of galvanism in cases of deafness.

IT appears, that galvanism is successfully applied in some cases of deafness; and particularly in such as cannot be relieved by other means. I am so fully persuaded of your desire to render the Philosophical Journal subservient to objects of utility, that I presume to request you to invite your correspondents, to transmit a particular account of the mode of applying galvanism in cases of deafness.

I am, Sir,

Your most obedient servant,

Feb. 1809. T.

V.

V.

An Addition Table, with a Multiplication Table on a new Plan. By Mr. CHARLES HAYTER.

SIR, *Dec.* 27, 1808.

MY son desired me to make him an *addition table*; the thought was new to me; but trying, I produced the enclosed, and since have done the multiplication table in the form herein presented. Several *teachers* have complimented the plan; because, when the Tables are learnt, the manner of setting down the sum is also perfectly taught, which is not the case with any other multiplication table. — Addition table. — Improved multiplication table.

The multiplication table of 144 squares is well adapted to stand at the beginning of duodecimals; and I have not the conceit to suppose the form I have produced will be any where preferable, but in the hands of very young beginners. Your opinion, Sir, of the composition will oblige

Your obedient servant,

CHARLES HAYTER

Portrait Painter.

42, *Margaret Street,*
Cavendish Square.

ANY means for facilitating the early acquirement of knowledge must be of value to society. It is certain, that though all educated persons can multiply figures very well, yet there are few, except those in counting-house, who can add with rapidity and certainty. This author's Table, if well fixed in the memory, appears to afford a remedy for the last evil. But I would observe, that the square multiplication table has the advantage of giving products readily when the factor is the largest of two numbers; so that a boy thus taught can as readily say 8 times 4 is 32, as 4 times 8 is 32, which it is well known cannot at first be done by one who has learned only the half-table with braces. It would be an improvement in both tables of Mr. Hayter, if they were thus completed. — Addition seldom well learned. — Complete tables best.

W. N.

AN

AN ADDITION TABLE.

One and one	1 1	2 1	3 1	4 1	5 1	6 1	7 1	8 1	9 1
make	2	3	4	5	6	7	8	9	10
Two and two	2 2	3 2	4 2	5 2	6 2	7 2	8 2	9 2	
make	4	5	6	7	8	9	10	11	
Three and three	3 3	4 3	5 3	6 3	7 3	8 3	9 3		
make	6	7	8	9	10	11	12		
Four and four	4 4	5 4	6 4	7 4	8 4	9 4			
make	8	9	10	11	12	13			
Five and five	5 5	6 5	7 5	8 5	9 5				
make	10	11	12	13	14				

A MULTIPLICATION TABLE.

one Twice	1 2	2 2	3 2	4 2	5 2	6 2	7 2	8 2	9 2	10 2	11 2	12 2
make	2	4	6	8	10	12	14	16	18	20	22	24
three Three times			3 3	4 3	5 3	6 3	7 3	8 3	9 3	10 3	11 3	12 3
make			9	12	15	18	21	24	27	30	33	36
				4 4	5 4	6 4	7 4	8 4	9 4	10 4	11 4	12 4
				16	20	24	28	32	36	40	44	48
					5 5	6 5	7 5	8 5	9 5	10 5	11 5	12 5
					25	30	35	40	45	50	55	60
						6 6	7 6	8 6	9 6	10 6	11 6	12 6
						36	42	48	54	60	66	72

Six and six	6 6	7 6	8 6	9 6
make	12	13	14	15
Seven and seven	7 7	8 7	9 7	
make	14	15	16	
Eight and eight	8 8	9 8		
make	16	17		
Nine and nine	9 9			
make	18			

NUMERATION TABLE.

Units 1 One

Tens 2 1 Twenty one.

Hundreds 3 2 1 Three Hundred and twenty one.

Thousands 4, 3 2 1 Four thousand, 3 hundred and 21.

Tens of Thousands 5 4, 3 2 1 Fifty 4 Thousand, 321.

Hundreds of Thousands 6 5 4, 3 2 1 Six Hundred and 54 Thousand 321.

Millions 7, 6 5 4, 3 2 1 Seven Millions, 6 Hundred and 54 Thousand, 321.

Tens of Millions 8 7, 6 5 4, 3 2 1 Eighty 7 Millions, 6 Hundred and 54 Thousand, 321.

Hundreds of Millions 9 8 7, 6 5 4, 3 2 1 Nine Hundred and 87 Millions, 6 Hundred and 54 Thousand, 321.

7 7	8 7	9 7	10 7	11 7	12 7
49	56	63	70	77	84
	8 8	9 8	10 8	11 8	12 8
	64	72	80	88	96
		9 9	10 9	11 9	12 9
		81	90	99	108
			10 10	11 10	12 10
			100	110	120
				11 11	12 11
				121	132
					12 12
					144

The intention of composing these Tables in their present form is to convey to the Learner a perfect idea of the forms of the Sums in the Rule, which no former Tables have done. Observe, each Article in the Tables is a perfect Sum in the Rule to which it alludes.

VI.

On the Cultivation of the common Flax, Linum Usitatissimum *of Linné, as an ornamental Plant in the Flower Garden. By Mr.* John Dunbar, *Gardener to* Thomas Fairfax, *Esq.**

Flax cultivated as an ornamental flower

THE Horticultural Society will perhaps honour with their attention a short paper, the object of which is to bring into cultivation the common *flax*, as an ornament of the flower garden, not merely as such, but with a view to the profit it will afford, at least to the servant, if not to the master; and the interest of the former can seldom be promoted in an honest way, without some benefit accruing to the latter. This plant, when so cultivated, like wax and honey, forms part of the natural riches of a country, and if it could supplant the cumbersome yellow *lupine* in our flower borders, the annual revenue arising from it would amount to several thousand pounds.

would add to the wealth of the nation.

Five persons supplied with linen from one garden in this way.

If gardening were in its infant state among us, a complete treatise on the culture of this plant might be necessary; but as this is not the case, only what is especially material will be noticed, with some directions how to prepare the plant after it is gathered. They are the result of several years experience, by which a family consisting of five persons has been supplied with all the linen they required.

A loamy soil best.

The soil of every flower-garden is always rich enough to produce good *flax;* but if it is loamy rather than sandy, the quantity will be nearly double: even in the fields, which can never be cultivated with the nicety of a gentleman's garden, I have observed the greatest crops in a loamy soil, and that they yielded an article superior in quality as well as quantity: for as the durability of the fibre depends in some measure upon its size, there can be no doubt that tall and vigorous plants are preferable to small ones.

Mode of disposing it for ornament.

There are various ways of disposing this plant so as to be exceedingly ornamental, but none more so than scatter-

* Trans. of the Horticultural Soc. p. 71.

ing

ing it in random parcels, or little clumps of from 10 to 20 plants, towards the back of the flower borders, and in the front of the shrubbery: for, without the summer proves uncommonly dry, it will attain to the height of three or four feet. If a temporary edging, or summer screen is wanting for any particular bed, it may be also employed for this purpose.

The seeds of good flax are short, plump, thick, very oily, and of a bright brown colour. The best season for sowing them, in most gardens, is February, or the beginning of March, when the general crop of hardy annuals are put in; but if the ground be sandy, and naturally dry, they should be sown in *October* or *November*. No more attention than what is necessary for the other flowers in the garden, which is keeping down all weeds while in the seed leaf with a hoe, will be requisite for this. As soon as the seed begins to ripen, and the plants turn yellow, pull the whole up by the roots, and lay it in bundles exposed to the full sun, if the weather is fine, to dry completely. Then pull the heads off, and shake out the seeds. Immediately after, it must be laid to macerate in a ditch, or pond of water, and kept under by a long piece of timber floating upon it. From five to ten days is the time necessary for its immersion, and after the fifth, it must be examined daily, taking especial care that it does not lie too long. As soon as ever you find the fibres are sufficiently macerated to separate from one another kindly, spread it out to dry upon a new mown meadow. When dry it must be again collected into bundles, and either sent to the flax dresser, or prepared for spinning at home by the gardener's wife. In many districts, this operation is well understood, and if carefully performed, homespun linen from such *flax* will last twice the time of most of the Irish linen that is now to be purchased in our shops.

Cultivation.

Gathering.

Maceration.

Preparation.

I believe it is a great errour to pull the *flax* so green as is commonly practised, and a still greater to soak it in water, before it is previously dried: for the fibres require twice the time to macerate sufficiently for separation in the dressing; a process by which they are considerably weakened.

Cautions.

VII.

VII.

Analysis of a Mineral Water near Dudley, in Worcestershire. By Mr. W. Weldon.

Mineral water near Dudley.

ABOUT two miles to the south of the town of Dudley, upon the estate of the Right Hon. Lord Dudley and Ward, is a spring of mineral water, that has been famous, in the surrounding country, from time immemorial, in various scrofulous and cutaneous diseases. It is said to have been used with great success in what are called obstructions of the abdominal viscera, and in various cutaneous diseases; but in all cases of scrofula it has been considered as an almost infallible remedy.

The high character that it has obtained, and the great success that is said to attend the use of it, in the diseases before mentioned, has induced an opinion in the neighbourhood, that it deserves to be more extensively known.

In a disease that under one form or other is so very general throughout Britain, it is very probable, that this mineral water may prove a valuable and useful remedy, and deserving of a more general attention than hitherto it has met with.

Like most other mineral waters it would be most advisable to administer it on the spot; but where circumstances render this inconvenient, it may be sent for and used at home.

I am not informed that any body, living on the spot, undertakes to answer orders for it; but if any physician or surgeon should be inclined to recommend it, or if any patient, who may wish to try it, should be at a loss how to obtain it, on a personal application, or by letter, post paid, I shall be ready to give the address of a person, who I doubt not will send it at a moderate expense.

Geology of the country.

I am not personally acquainted with the geology of the surrounding country. I am told it is very interesting. It appears to be of what Werner names the independent coal formation. Coals occur in great abundance; one stratum

tum particularly is said to be ten yards in thickness; with iron stone, basaltes, clay, &c.

The saline spring flows into a well, near a ridge of high land, on the sides of which, at some short distance, coals and iron stone are seen cropping out. The well is about 36 feet in depth, and $7\frac{1}{2}$ feet in diameter. The sides have lately been fenced, to keep out foreign water, which was supposed to run into it, with a dam of bricks set in clay, and lined with elm boards. The spring.

The bottom is a ferruginous argillaceous sandstone, through which is perforated a hole, whence the water issues, and rises to within about four feet from the surface.

The sides of the well near the top are covered with a yellowish ochrey substance. When the water is fresh taken up, it is perfectly transparent and colourless. It is little refractive of light, nor can it be said to sparkle; but after standing for a short time, numerous small bubbles of air are seen adhering to the bottom and sides of the glass it is taken into. After a time the water becomes rather turbid, and at length a pale ochreous precipitate falls down, leaving the supernatant water transparent. The water.

In large quantity the water smells of sulphuretted hidrogen, but if half a pint or less be examined apart, the odour is hardly perceptible. Its smell.

When the temperature of the atmosphere indicated 40° of Fahrenheit, some water just taken from the well raised the thermometer to $47\frac{1}{2}$°. At another time, when the temperature of the surrounding air was 75°, water taken from near the surface of the well lowered the thermometer to 56°, and a portion taken from the bottom to 52°. Temperature.

The taste of the water very much resembles the taste of sea water. Taste.

The specific gravity of this water is found to differ at different depths from the surface. It differs also very considerably at different times. The following are the dates and specific gravities. Specific gravity.

July 18, 1806, taken from the bottom of the well sp. gr. to dist. water as 1018 to 1000.

Jan. 23, 1808, from the bottom of the well sp. gr. as 1028 to 1000.

Oct.

Oct. 27, 1808, sp. gr. as 1013·5 to 1000.

Dec. 2, 1808, two portions of the mineral water were sent to me, one of which was taken from about the middle of the well, the other from the bottom.

Sp. gr. of the water from the bottom 1013·5.

Sp. gr. of the water from the middle 1012.

Foreign water was excluded.

In consequence of some freshwater springs being found to discharge themselves into the well, it has lately been emptied and walled round as above stated; and it is probable that hereafter the strength of the water will be more regular.

Action of the water upon lime.

It seems, that the workmen experienced a great deal of difficulty, in securing the dam. At first, they built the dam with common lime mortar, the action of the water destroyed its cementing power. Then a fresh wall was built, with a gray lime, which the neighbourhood affords, and which sets very firm under water; but this also, as might be expected, failed, from the action of the water on the lime. At length the dam was formed by setting bricks in a strong clay, that is free from lime or nearly so, and it seems to answer the purpose intended.

Examination of the water from the bottom of the well.

The mineral water that was subjected to the following experiments was taken from the bottom of the well, by sinking empty bottles provided with valves, as soon as the bottles were drawn up, they were carefully corked, and tied down with bladder.

Its appearance.

1. A glass bottle full of this water was perfectly transparent, and without sediment while uncorked. After standing for some time in the laboratory, where the thermometer stood at 40°, the cork was drawn, a small portion of gas compressed in the neck of the bottle escaped, but no bubbles of air were seen to escape or even to form for some minutes after. At length however the water became turbid, and gradually deposited an ochrey sediment.

Tested with tincture of red cabbage:

2. *a.* Pure distilled water was tinctured with a certain proportion of tincture of red cabbage, and placed as a standard of the colour.

b. An equal quantity of the tincture of cabbage was added to a corresponding portion of the mineral water, which was quite fresh. At first it became slightly reddened, but

but the tint gradually went off, and in three hours the colour was distinctly blue.

c. An equal quantity of the mineral water, which had been boiled and filtered, produced no change of colour of the tincture of cabbage at first, but when examined ten minutes afterward it was slightly blued.

3. a. Pure distilled water coloured with a certain portion of tincture of litmus as a standard. litmus:

b. The fresh mineral water in the same proportion produced the slightest tint of red. It soon vanished.

c. The mineral water which had been boiled and filtered produced no change.

4. a. Pure distilled water was tinged with infusion of rhubarb in given proportion as a standard. infusion of rhubarb:

b. The fresh mineral water tinged with an equal proportion was very slightly deepened.

c. The boiled water tinged as above could not be distinguished from pure distilled water.

5. Corresponding experiments were made with infusion of brasil. Both the fresh and the boiled mineral water produced the slightest tint of blue. infusion of brasil:

6. Sirup of violets was changed to a green by the fresh water. The boiled water produced no change on it. sirup of violets:

7. a. A few drops of infusion of galls added to the fresh mineral water produced a violet colour, the water gradually became opaque, and in twenty-four hours had thrown down a rather copious precipitate of a violet red colour. The liquid above was transparent and colourless. infusion of galls:

b. The infusion of galls produced no change in the mineral water after boiling.

8. a. Lime water in very small quantity produced a pale ochrey precipitate in the fresh mineral water. limewater:

b. On adding to the fresh mineral water an equal part of lime water, a white precipitate formed, which dissolved again in nitrous acid.

9. Strong sulphuric acid rendered the fresh water turbid, and disengaged a number of air bubbles. An abundance of sulphate of lime formed. sulphuric acid, strong

10. A few drops of diluted sulphuric acid was added to some of the mineral water, which had been boiled and filtered, and diluted:

tered, and was previously warmed. No flavour of sulphuretted hidrogen was perceptible.

muriatic acid: 11. The mineral water previously boiled was treated, as in 10, with muriatic acid; no change was produced in its transparency, nor was any flavour perceptible except of the muriatic acid.

leaf silver: 12. Silver leaf left in contact with a portion of the fresh water, in a closed phial, for several days, was not perceptibly tarnished.

nitrate of mercury: 13. Nitrate of quicksilver produced a copious white precipitate.

oxide of bismuth: 14. *a.* Oxide of bismuth mixed with the fresh water in the proportion of 4 grains to 4 oz. was very slightly darkened.

b. With the boiled water no change of colour was produced.

acetate of lead: 15. *a.* Acetate of lead threw down from the fresh water a dense white precipitate. When the precipitate had wholly subsided, the surface of it was of a pale ochre or fawn colour, apparently produced by the oxide of iron only.

b. With the mineral water, after being exposed to the atmosphere for a few hours, and decanted, the precipitate was a pure white.

nitrate of lead: 16. Nitrate of lead threw down a dense precipitate of a pure white colour.

nitric acid: 17. Nitric acid produced no change in the fresh water.

nitrate of silver: 18. Nitrate of silver produced a dense white precipitate, which was kept for several days in a dark room, without undergoing the least change of its colour.

acetite of silver: 19. *a.* A few drops of acetite of silver threw down a precipitate of a rather dull white colour. It was kept in a dark room for 24 hours, when it appeared somewhat darkened.

b. The boiled water treated in the same manner did not darken the acetite of silver.

muriate of mercury: 20. *a.* Muriate of quicksilver, added to the fresh water, produced no immediate change. After a few minutes the water became slightly opaque, and a slight precipitate at length appeared. It scarcely exceeded the quantity of oxide of iron which the water contained, but the colour was of a redder

a redder tint than the ochrey powder which the water let fall spontaneously.

b. The same salt added to some of the mineral water which had been exposed to the atmosphere for some days, and decanted, produced no change in it.

21. Oxalate of ammonia instantly produced a copious white precipitate. oxalate of ammonia:

22. Oxalic acid produced the same. oxalic acid:

23. A solution of pure ammonia threw down a precipitate. pure ammonia:

24. Equal parts of the mineral water and of fresh made lime water were mixed together, and the precipitate that was thrown down was separated and washed. It was dissolved in muriatic acid, precipitated by subcarbonate of soda, washed, and dried in a low heat. Distilled vinegar dissolved a part of it. A solution of pure potash dissolved the greater part of the remainder, leaving a brown powder. lime water:

25. Acetate of barytes produced no precipitate in the filtered water. acetate of barytes:

26. Muriate of barytes produced no precipitate in 24 hours. muriate of barytes:

When the saline contents of the water were concentrated by evaporation to one fourth, a slight precipitate formed.

27. Muriate of strontian produced no precipitate in the water, either when fresh, or when concentrated as in 26. muriate of strontian:

28. Phosphate of soda produced a copious precipitate. phosphate of soda:

29. Carbonate of ammonia produced a white cloud. carbonate of ammonia.

30. A crystal of carbonate of potash did the same. carbonate of potash:

31. *a.* In the fresh water succinate of ammonia produced a reddish brown precipitate. succinate of ammonia:

b. In the boiled water a few drops of succinate of ammonia produced no change.

32. *a.* Prussiate of potash and iron produced a deep blue precipitate in the fresh water. and prussiate of potash.

b. In the boiled water this triple salt produced no change.

33. A portion of the fresh water was boiled in a glass retort, the curved arm of which was placed under an inverted jar filled with quicksilver. The gas which was thus collected was tried with nitric acid, the acid absorbed a part of it. Gas expelled by boiling and tested with nitric acid,

34. A

superacetate of lead,

34. A portion of gas produced as in 33, was exposed to the action of a solution of superacetate of lead, by which a part of it was absorbed.

& lime water.

35. Another portion of this gas, and also the residua of the experiments 33 and 34 were severally exposed to the action of lime water. A corresponding proportion from each of the gasses was absorbed.

Precipitate from boiling treated with muriatic acid.

36. The precipitate obtained by boiling a quantity of the fresh mineral water to about one half was washed, dried, and treated with dilute muriatic acid. A part of it only was dissolved.

37. A solution of pure ammonia threw down a white precipitate from the muriatic solution in 36.

38. Oxalate of ammonia occasioned a precipitate in the same solution.

The residuum examined.

39. The powder which resisted the action of the muriatic acid in 36 was boiled in subcarbonate of soda, and then in a solution of pure potash. A part of it was dissolved by each.

40. The powder left undissolved by the last operation was boiled in muriatic acid. What remained was melted in a large proportion of pure potash. Silex was precipitated on the addition of distilled water.

The water evaporated to dryness.

41. A quantity of the fresh water was evaporated to dryness. The dry mass was treated with successive portions of alcohol, the specific gravity of which was 815°. The residuum, which resisted the action of the alcohol, was dissolved in water, filtered, and slowly evaporated. The crystalline substance thus successively separated, was pure common salt.

Contents of the water.

From these experiments it appears, that this mineral water contains a small proportion of sulphuretted hidrogen, the same of free or rather supercombined carbonic acid, carbonate of iron, carbonates of lime, of magnesia, and of argil, probably carbonate of manganese also, a minute proportion of sulphate of lime, with muriates of soda, lime, magnesia, and of argil, silica, with azote, or atmospheric air.

Their proportions examined.

To obtain an approximation to their respective proportions, the following additional experiments were made.

42. Grains

42. Grains by weight 22045 = 85·973 cubic inches nearly of the fresh mineral water were poured into a flask, and securely luted to a tube terminating under a graduated jar filled with quicksilver. The quantity of atmospheric air that was contained above the water in the flask and in the tube was ascertained to be 9·5 cubic inches. The thermometer was at 47° of Fahrenheit, and the barometer 29·7. The water was boiled for about half an hour. When the apparatus was cold, the whole of the air in the flask and tube was transferred into the graduated jar. When reduced to the same temperature and barometrical pressure as before the operation of boiling, it measured 16·1 cubic inches. Air expelled.

43. The gas thus collected was washed in a solution of superacetate of lead, until no farther absorption took place. The quantity absorbed was 1·95 cubic inches. Tested with superacetate of lead:

44. The gas was then exposed to the action of fresh made lime water, until it underwent no farther diminution of bulk. The quantity absorbed by the lime water was 2·1 cubic inches. lime water:

45. The residual gas amounting to 12·05 was next treated with a solution of sulphuret of lime, that had been saturated with azote. The quantity of oxigen absorbed was 1·9 cubic inches. and sulphuret of lime.

The 9·5 cubic inches of atmospheric air contained in the apparatus consisted of 7·5 + cubic inches of azote, and 2 — cubic inches nearly of oxigen. The whole of the oxigen indicated in 45 was contained therefore in the atmospheric air. The small deficiency of oxigen appears to have been produced by that quantity having combined with the black oxide of iron, which during the experiment fell down in the form of red oxide.

2·65 cubic inches of azote therefore were given out by the quantity before stated of the mineral water.

46. 88180 grains by weight = to 343·9 cubic inches of the fresh mineral water were boiled for an hour or more and filtered. They were then evaporated to about 30 cubic inches, when the muriate of soda began to crystallize. The water was then filtered again, and the small quantity of precipitate that was then obtained, and which from a pre- The water evaporated. Muriate of soda crystallized.

 vious

vious examination was known not to contain any sulphate of lime, was added to the former.

Precipitates treated with muriatic acid, and caustic alkali.

47. The precipitates obtained from the mineral water by 46 were treated with strong muriatic acid, and boiled in it, which did not completely dissolve them. The undissolved powder was washed and boiled for some time in a silver crucible, with a strong solution of pure potash. The solution was diluted, the powder separated, and again treated with muriatic acid, washed, and dried in a red heat. This powder weighed gr. 2·6.

The solution precipitated, & the precipitate examined.

48. The washings were all added to the solution formed in 47, and the whole precipitated by subcarbonate of soda. The supernatant solution was neutralized and the precipitate washed. It was redissolved in muriatic acid. The muriatic solution was neutralized, diluted, and treated with prussiate of potash and iron. The precipitate thus formed was calcined to destroy the prussic acid; nitric acid was abstracted from it two or three times; and then it was dissolved in muriatic acid. The solution was high coloured, it was diluted with water, and precipitated by carbonate of potash. The precipitate was separated by filtration, washed and dried. The subcarbonate of iron thus obtained weighed gr. 5.5.

Subcarbonate of iron.

49. The liquor left after filtering off the subcarbonate of iron in the last operation, was boiled for three quarters of an hour or more, a light coloured precipitate after some time began to separate, which soon became darker coloured. This powder, which I conclude to be carbonate of manganese, when washed and dried weighed gr. 0·75.

Carbonate of manganese.

50. The muriatic solution that was filtered off from the prussiate in 48 was accurately neutralized. Oxalate of ammonia was then dropped in gradually, as long as a precipitate formed. The oxalate of lime, when washed and dried, weighed gr. 3·1 = gr. 1·5 of pure lime, or gr. 3·1 of carbonate of lime.

Carbonate of lime.

51. The solution left in fifty was then treated with pure potash. The precipitate was separated, washed, redissolved in muriatic acid, and again precipitated by subcarbonate of soda, washed, and dried in a heat of near 200°. It weighed gr. 19·75.

52. This

52. This precipitate was treated with distilled vinegar, until the vinegar no farther acted upon it. The residuum, when washed and dried, weighed gr. 3·2. Consequently gr. 16·55 of carbonate of magnesia were dissolved by the vinegar, and gr. 3·2 of subcarbonate of alumina were left.

Carbonate of magnesia, and subcarbonate of alumine.

53. The concentrated mineral water, which was separated from the earthy carbonates &c. in 46, was diluted with distilled water, and oxalate of ammonia added, until the whole of the lime fell down. The oxalate of lime that was thus obtained was well washed and dried. It weighed gr. 482·25 = to gr. 241 of pure lime = to gr. 482·25 of dry muriate of lime, taking Kirwan's estimate for the composition of the latter.

Muriate of lime.

54. Pure potash was now added to the filtered solution left in 53. The precipitate was separated, redissolved in muriatic acid, and precipitated by subcarbonate of soda. The precipitate washed and dried weighed gr. 118·3.

55. This precipitate was treated with distilled vinegar until no more dissolved. The subcarbonate of magnesia thus dissolved weighed 99·05, equal to gr. 143·45 of muriate of magnesia according to Kirwan. The alumina when heated red hot washed and dried weighed gr. 17·25 = to nearly gr. 57·3 of muriate of alumina.

Muriates of magnesia and alumine.

56. 88180 grains of the fresh water were evaporated to dryness. A quantity of muriatic gas escaped towards the end of the operation from the partial decomposition of the muriates of magnesia and alumina. Water and a small quantity of muriatic acid were now added to dissolve the soluble parts of the mass; the insoluble powder was then treated with strong muriatic acid, washed, dried, and weighed. It was now thrown into a hot solution of subcarbonate of soda, from which it was separated, and treated with diluted muriatic acid, which dissolved a minute proportion of carbonate of lime. This was precipitated and separated by a filter, but did not exceed 0·25 of a grain by computation.

57. The insoluble powder, which could consist only of silica and alumina, was boiled in a silver crucible with a solution of pure potash, the solution was decanted off, and the

Silex.

white powder that was left was washed, and exposed to a red heat. It weighed gr. 3.

Sulphate of lime.

58. In order to obtain a nearer approximation to the quantity of sulphate of lime, contained in a given portion of the mineral water, than that which was obtained in 56, where in dissolving the muriates a part of the sulphate was probably redissolved, 22045 grains of the mineral water were evaporated to about one eighth of their bulk, and filtered. A known proportion of muriate of barytes was then added. A slight cloudiness was perceived in a few minutes. Twenty-four hours after, the small quantity of precipitate, that had formed, was separated by a filter, washed, and dried. Allowing for what was lost upon the filter it amounted to 0·25 nearly.

59. 22045 of the mineral water were exposed for some days to the atmosphere and filtered: nitrate of silver was then added, until no farther precipitation ensued. The whole was poured upon a filter, and the precipitate was well washed, and dried for twenty-four hours in a stove at 212° nearly. The muriate of silver thus formed weighed 937·25 grains, which multiplied by 4 give 3749 grains for the quantity of muriate of silver formed by muriatic acid contained in 88180 of the mineral water.

60. If we suppose the muriate of silver to contain 18 per cent of acid; the quantity of muriatic acid would amount to 674·82 grains.

If grs. 482·25 of muriate of lime contain gr. 202·545
143·45 of muriate of magnesia . . . 81·766
57·3 of muriate of alumine . . . . 17·19

Muriate of soda.

of muriatic acid; there will be gr. 787 of muriate of soda, supposing it to contain 50 per cent of base, 44 of acid, and 6 of water.

The muriate of soda found in a different way.

61. A given quantity of the mineral water was evaporated to dryness, and digested in alcohol of ·815 applied in successive proportions to dissolve the earthy muriates. The muriate of soda thus obtained was decomposed by nitrate of silver. The result of this experiment was nearly as above. The following therefore may be considered as a near approximation to the quantity of gaseous and solid contents contained

contained in 88130 grains or 343·9 cubic inches of the mineral water.

Contents of the water.

	Cubic Inches
Of sulphuretted hidrogen	7·8
carbonic acid	8·4
azote	10·6

	Grains.
Of muriate of lime	482·25
magnesia	143·45
alumine	57·3
soda	787·
Of sulphate of lime	1·07
carbonate of lime	3·1
of magnesia	16·55
subcarbonate of alumina	3·2
subcarbonate of red oxide of iron gr. 5·5 nearly equal to 5 grains of the subcarbonate of black oxide	5·
Of subcarbonate of manganese	·75
Of silica	3·

Proportions in a gallon.

Hence one gallon, or 231 cubic inches of the water, contained

	Cubic Inches.
Of sulphuretted hidrogen	5·24
carbonic acid	5·64
azote	7·1

	Grains.
Of muriate of lime	323·93
magnesia	96·35
alumine	38·5
soda	528·63
Of sulphate of lime	0·71
carbonate of lime	2·1
of magnesia	11·12
Of subcarbonate of alumine	2·15
of black oxide of iron	3·36
of manganese	·475
Of silica	·2

I have

At different times the water differs in gravity,

I have before observed, that this mineral water has been found to possess different specific gravities at different times, consequently to contain at different times different quantities of ingredients.

and in the proportions of its ingredients, particularly the iron.

The specimen of the water that was sent to me in July 1806, the specific gravity of which was 1·018, was submitted to some experiments, which show, that it differs also at different times in the proportion of its ingredients. In this case the difference is very remarkable in relation to the oxide of iron.

Instance in a different portion examined.

When boiled in a retort that terminated under quicksilver, the black oxide of iron fell down in abundance, and the earthy carbonates were in very small proportion. The water that was decanted threw down a green hydrate of iron mixed with magnesia, on the addition of lime water. When the water was boiled in the open air and filtered, lime water threw down a brown precipitate.

The following is the proportion of some of the ingredients it contained. While this examination was proceeding I was obliged to suspend it for a time, to attend to more pressing avocations, and an accident prevented my completing it.

Its contents.

From a wine gallon, or 231 cubic inches, were obtained

Of muriate of soda	483·
lime	311·
magnesia & alumina	145·
iron	26·
Of carbonate of iron	9·
Of silica	·75
Of earthy carbonates about	4·5
	Cub. In.
Of carbonic acid and sulphuretted hidrogen (the latter in small proportion)	23·735
Of azote	12·

The ingredients probably not in the exact state here assigned.

In both of the foregoing statements it will be seen, that I have given the ingredients as they occurred on decomposition.

But it by no means follows, that they exist in that state when in solution. It is more probable, that when in solution

tion the earths and oxides are combined with the acids, as is observed by Mons. Berthollet, in proportions depending upon a power produced by the respective proportions and the respective affinities of all the ingredients toward each other.

10, *Wigmore Street, March*, 8, 1809.

VIII.

On the Gold Mines in the Department of the Isère. By HERICART DE THURY, *Mine Engineer.*

(Concluded from p. 237.)

IT is difficult to meet with a vein so regular and well defined as that of Gardette. In fact it shows itself both on the surface and within the earth for a uniform length of 450 met.; and throughout the whole of this extent it retains exactly the same direction of SSE and NNW, without being broken off or turned aside by any failure or vein appearing at the surface. Description of the vein.

The inclination to the depth of 78 met. [85 yds] to which the last shaft is driven, is pretty regular thoughout its whole height, being an angle of 80°. It has experienced only two slight variations; the first, near the surface, is a slight break of 15 or 20, or at most 25 cent. [6, 8, or 10 inches] with the same direction and inclination as the vein. The second has been found at 62 met. [68 yds.] deep; and is occasioned by a small vein composed of lead, copper, and sulphuret of zinc, which has turned the principal vein toward the WNW about 3 met. [near 10 feet] in that part of the vein which serves as its roof. Below this the slope has become more rapid, though its direction remains the same.

In all the workings the vein of la Gardette has been found to consist of compact quartz, which is crystallized wherever the silex did not fill up the whole cavity. This quartz affords very various groupes, which are very limpid when divested of the oxide of iron that covers them. The quartz constitutes the bulk of the vein, but various different metallic Gangue.

lic substances are found in it, such as sulphuret of lead, phosphated lead, arseniated lead, earthy oxide of lead, argentiferous gray copper ore, yellow pyritous copper, arseniated copper, green carbonate of copper, iron spar, sulphuret of iron, oxide of iron, acicular oxide of manganese, tellurium, &c. These substances are distinct, or combined two, three, four, or even five together. Frequently they contain gold, and sometimes this metal is apparent in them.

State of the gold at Gardette. Native pure.

The gold is disseminated in the quartz of the vein. It is native and pure, or mixed, or alloyed with different substances. It occurs without mixture in four different forms. In octaedral crystals, adhering one to another so that it is frequently difficult to ascertain their figure. 2. In ramifications, or dendritical, the most distinct specimens of which appear to consist of little octaedra implanted one on another. 3. Capillary slender filaments between the quartz crystals. 4. Laminar, sometimes in flat, at others in twisted laminæ, the surface of which is occasionally reticulated, but more frequently like shagreen.

Mixed.

Of the apparently native gold mixed with other metals there are three varieties. 1. Ramified or capillary in crystals of sulphuret of lead. On breaking these we perceive ramifications and filaments of gold of a brilliant yellow colour. 2. Granular in sulphuret of lead. 3. Native gold in sulphuret of zinc.

Gold alloyed with other materials.

The native gold alloyed with other metals, and concealed by them, may be distinguished into eight varieties. 1. In a mixture of lead, copper, and sulphuret of zinc. 2. In the argentiferous gray copper ore with green carbonate of copper. 3. In sulphuret of iron. 4. In the sulphurets of zinc, lead, and copper, covered with arseniate of copper. 5. With tellurium in needles. 6. In oxide of iron. 7. In phosphate of lead. 8. In oxided manganese.

Gangues of the native gold.

The native gold is found likewise in five different earthy gangues. 1. In limpid hyaline quartz, or rock crystal. 2. In smoky hyaline quartz. 3. In black hyaline quartz. 4. In hyaline quartz with sulphate of barytes. 5. In hyaline quartz and carbonate of lime or calcareous spar.

Products of the gold mine.

The workings of the gold vein of la Gardette have afforded three different productions. 1. The gold which has been

been cast into ingots, and sent to Paris, to the count of Provence, in 1786. 2. The gold ore, which, not being sufficiently rich for the furnace, and affording instructive specimens for the mineralogist, was set aside at the suggestion of Mr. Schreiber, to be sold according to its intrinsic value to curious and scientific collectors, as is customary in the mines of Saxony and Austria. 3. The rock crystals which have been collected in the cavities of the quartz.

The processes followed in the extraction of the gold are simple, and easy to conceive. The native gold requires only to be separated from its gangue and united together. For this purpose it is pounded; washed, to carry off the gangue reduced to fine powder; and triturated with ten or twelve times its weight of mercury in a mortar filled with water. This water being decanted off carries with it every thing earthy. The amalgama being separated from the earth, and perfectly brilliant, it is pressed in leather bags, to strain out the superfluous mercury from what is requisite to dissolve the gold. The solid amalgama left behind is distilled in retorts, which are brought to a strong red heat, to obtain the mercury separate; and the gold, which remains behind, is melted and cast in bars or ingots. Method of extracting the gold.

The gold combined or disseminated in the ores of silver, copper, lead, &c., is extracted by eliquation, cupellation, and parting. The lead that runs during the eliquation of the argentiferous and auriferous copper carries with it the silver and the gold. This mixture is cupelled to scorify the lead. The gold and silver that remain are separated from each other by parting with nitric acid.

It has been proposed, to treat the auriferous pyrites by amalgamation, the success of which, as already confirmed in Germany and Peru, proves, that the gold in them is simply disseminated, and not combined.

The working of the gold mine of la Gardette has been suspended ever since the year 1788, yet it ought not to be given up. It is at present under the superintendance of the mayor of Villard-Eymont. The distance, difficulty of access, complete ruin of the miner's house, falling in of the earth, and general decay of the mine, has occasioned it to be neglected by the local authorities; and the inhabitants of Not wrought since 1788.

of the village of Gardette annually avail themselves of this desertion, as during the dead time of the year they search for gold at their own expense, and are frequently successful.

Gold sands of the Rhone.

The Rhone has been frequently quoted for its golden sands. The most ancient authors, as Pliny, Diodorus Siculus, Strabo, and Polybius, speak of the gold dust, which this river rolls along with its sands; and which the Gauls knew how to wash so as to extract the gold, of which they made rings, bracelets, and belts. Reaumur, after having given the history of rivers and rivulets with auriferous sands in the Memoirs of the Academy of Sciences for 1778, says, that the gold collected in the Rhone is 20 carats fine, containing one sixth of its weight of copper and silver.

I shall now proceed to the mines of gold alloyed with different metals, by which its presence is concealed.

1. *Gold in the sulphuret of lead of Pontraut.*

Gold in sulphuret of lead:

Pontraut is a part of the chain of granitic mountains known by the name of *Petites Rousses,* above Oz and Vaujani in Oisans. This vein is near the glaciers; it is more than two hours journey from the villages abovementioned, and in a country so cold, that it is inhabitable only four months of the year at most. The ore of Pontraut yields to the essay 58 per cent of lead: and this lead contains 122·286 gr. of silver, and 1·442 of gold, in 50000.

2. *Gold in the Sulphuret of lead of Mollard.*

another:

Mollard is a village of the commune of Allemont, situate on the right bank of the river Olle. The mine was opened by Mr. Schreiber in 1785 for the smelting works of Allemont.

This ore yields 60 per cent of lead: and 50000 gr. of the lead contain 61·143 of silver, and 1·272 of gold.

3. *Gold in the sulphuret of antimony of Auris in Oisans.*

in sulphuret of antimony:

The ore is a mixture of lead, zinc, copper, antimony, silver, and gold, united and intimately mixed. It is frequently coloured by green carbonate of copper.

It yields 50 per cent of antimony: and 10000 gr. of this antimony

antimony are said to contain 950 of silver, and 4·812 of gold.

4. *Gold in the yellow copper pyrites of la Cochette.*

This mine is in the narrow passage of la Cochette, which forms a communication between Vaujani in Oisans and Saint Sorlin in Maurienne. The height of the mine, and its difficulty of access, will never allow it to be worked with advantage. It appears, that it was attempted formerly, and a tradition of the fact is preserved; but, as is too commonly the case, the narrative has a great deal of the marvellous mixed with it. in copper pyrites:

The ore of la Cochette yields 36 parts of refined copper, and ·00236 of gold, from 100 of black copper.

5. *Gold of Theys in a pyritous copper.*

This mine is in la Combe-de-Merle, below the lake of Seche-Dent, on the western slope of the mountain of Theys, below some mines of iron spar, and in a forest of pines. It consists of nodules of auriferous copper pyrites disseminated irregularly in a vein of iron spar. another:

The ore has never been accurately analysed. Yves Michael du Serre, in a remonstrance to the Duke of Orleans published in 1651, says, "it is so pure and clean, that four parts yield three of the finest gold, and it is as plentiful as possible."

6. *Gold of Alleyard in an argentiferous gray copper ore.*

This ore is frequently in a state of decomposition, and coloured by green and blue carbonate of copper. It is found in nodules in a vein of iron spar, at Buisson near Allevard. in argentiferous gray copper ore.

A hundred parts of the ore furnish 60 of black copper, which yield 38 of refined copper, 4 of silver, and ·003158 of gold.

There is reason to suppose, that this is the mine of which Hellat speaks in his *Etat des mines du Royaume*, when he says, that Mr. de Baral, proprietor of the iron mines of Allevard, had found a fine gold mine in that district.

7. *Gold*

7. Gold in the yellow copper pyrites of Chalanches.

in copper pyrites.

This mine is situate above the confluence of the Romanche and the water of Olle, in the commune of Allemont. It is celebrated in the annals of French mineralogy for the richness of its veins of silver*.

We do not know the proportions of the principles of this ore, which was analyzed by Schreiber, who found gold in it.

IX.

A short Account of Nectarines and Peaches naturally produced on the same Branch. By R. A. SALISBURY, *Esq. F. R. S. &c.*†

Nectarines and peaches sometimes produced on the same branch.

THOUGH it has long been known, that *nectarines* and *peaches* are sometimes naturally produced, not only upon the same tree, but upon one and the same branch, I do not find the fact recorded by any author; and having last year met with two instances, I presume to offer a short history of this anomaly to the Horticultural Society: whether the remarks it has suggested are right or wrong, I leave to be determined by more able physiologists.

Earliest notice of this.

The first instance, of which I believe any tradition has been handed down, will be found in a letter of the late Peter Collinson Esq. to Linne, which was read at the last meeting of the Linnean Society. He there, after giving an account of a supposed adulterous intercourse between two *apple* trees, standing near each other, one of which in consequence bore both smooth and rough fruits, mentions a *peach* tree, that produced *peaches* and *nectarines*.

Smooth and rough apples.

Another instance.

The second instance occurred in *Yorkshire*, at *Londesborough*, then the residence of the Earl of Burlington; it made so much noise at the time, which was previous to the death of that famous gardener, Thomas Knowlton, as to be visited

* See our Journal, p. 124 of the present vol.

† Trans. of the Horticultural Society, p. 103.

by

by the late Dr. Richardson, and many other horticulturists of that extensive county.

The third instance is commemorated by a painting of the celebrated Ehret, now in the possession of Messrs. Lee and Kennedy: being accompanied with a dissection of the two fruits, which are the *alberge jaune*, sometimes called the *orange* peach, it is very satisfactory. 3d.

The fourth instance was noticed more lately in the garden of William Gilpin, Esq., *East Sheen*; of this likewise a painting, but without dissections, has been made by Mr. Hooker, nor can I from it ascertain the variety. 4th.

The fifth instance was discovered early in *June* last, on the wall of Sir John Arundel at *Huntingdon*: having never seen one, I went there immediately, and after detaching the branch carefully from the wall, soon satisfied myself that no bud had been inserted: there was however only a single *nectarine* upon the tree, which the gardener said was the *belle chevreuse*, and a pretty accurate sketch of the branch is annexed *. 5th.

The sixth instance was in Mr. Wilmot's garden at *Isleworth*, which I also saw in *August* last, and learnt that his tree, which is the *royal George*, seldom fails to produce fruits with both smooth and downy coats, or in fact *peaches* and *nectarines*: two only of the latter then remained, and had been much damaged by snails. 6th. Tree usually producing both.

I forbear to recite any others, these being more than sufficient to establish the truth; but my inquiries fortunately terminated with the singular example now before you, of both fruits joined in one. I have to thank Dr. Batty for it, who accidentally observed it among a number of *peaches*, sent to him by James Wyatt, Esq., from the neighbourhood of *Hounslow*, during our vacation; and as it was already beginning to decay, this only method of preserving it for your inspection was not neglected. Instance of the two fruits united.

* On this branch, the bearing wood of which is about a foot in length, there are two peaches, eight inches distant from each other, and between them is a nectarine. I did not think it necessary, to have it reengraved. A figure is likewise given of the fruit next mentioned, one part smooth, the other downy.

Most

Not as supposed owing to the pollen brought from nectarines by bees.

Most of the gardeners, with whom I have conversed respecting these anomalies, attribute them to the *pollen* of neighbouring *nectarine* trees brought by bees: but, as the young fruit is smooth or downy long before it is impregnated, this cannot be the cause; and in my humble opinion, no change of this sort is produced subsequently. Not that I have a shadow of doubt of the important consequences which ensue when the *stigma* of one plant imbibes *pollen* belonging to another; but these are only manifested in the succeeding generation. The great Linne, in the *Plantæ Hybridæ* and *Generatio Ambigena* of his *Amænitates Academicæ*, first promulgated a doctrine, which I firmly believe, that varieties, species, and even genera, have been created in this manner; and without the fullest comprehension of it no gardener can hope to be successful in raising new vegetables, free from the faults, or endowed with the perfections he wishes. The pith of Linne's theory is, that the new vegetable will resemble its father, or that from which the *pollen* came, in stem and leaves; but its mother, or that upon which the *stigma* is situate, in flowers and fruit; this idea, which somewhat less restricted has been confirmed by actual experiments, should never be forgotten. Of the necessity of a sexual intercourse, every one who has raised a *cucumber* or *melon* is well convinced, and as far as the annual production of these or other fruits is concerned, I have nothing to hint in addition to modern practice, except that the *pollen* of all vegetables might probably be preserved from one year to another; in early forcing, it would be found very useful, and should be kept in papers as dry as possible, not applying it till the *stigma* is moistened with its own natural exudation. In those countries, where *dates* are the principal food of the inhabitants, a famine would sometimes be the consequence of neglecting this precaution; for the male trees do not flower every year, and it is well authenticated, that *pollen* of this *palm* performed its office successfully, after being sent many miles by the post to *Berlin*.

Linneus's theory of the production of new kinds of plants.

Pollen may be preserved.

Other vegetables vary in their pubescence.

Other vegetables sport in their pubescence as remarkably as this, but being of less importance, are not attended to. Two years ago I observed a *wall-flower-leaved stock* with both smooth and downy leaves, in Messrs. Whitley and Braine's

Brame's nursery. The common *ling*, of which our besoms are made, varies in the same way; and the *teucrium heterophyllum* takes its name from this very circumstance. I conclude therefore, that all these variations proceed from laws in vegetation, of which we are yet ignorant, but which are immediately connected with the transudation of the sap through the cuticle, and it is possible, that this may even affect the flavour of two fruits upon the same branch.

Laws in vegetation unknown to us.

X.

On the Substitution of Iron for Mahogany and other expensive kinds of Wood in Articles of Furniture, and for other Purposes. *By Mr.* B. Cook.

To Mr. NICHOLSON.

SIR,

As you have favoured me with inserting in your valuable Journal my former imperfect communications, I have taken the liberty, to lay before you my idea on another subject; and I leave it to you to judge, whether the idea is worth communicating to the world.

We import at a great expense mahogany and other costly woods for the manufacturing of the very beautiful furniture in use among the higher and middling ranks of society. The great advance of this article is felt by every one, who finds it necessary to purchase things made of mahogany. If it were possible to find a substitute for a portion of this article, were it only the half, or even a fourth part of it, it would certainly reduce the price of that part, which necessity forces us to use, in a given ratio. People of the highest order of fashion will never submit to use a substitute, unless this substitute is equal in beauty to that, the place of which it supplies. But if it were possible to find an article equal in beauty, more durable, and as cheap, we should be inclined to think it would be adopted in every case, where it was found convenient; especially if this substitute were a native of our own country.

Mahogany and other wood imported at a great expense.

Now

Iron abounds in this country.

Now Great Britain produces iron in abundance. I do not pretend to assert, that iron is the best substitute: if any thing else can be discovered, which will answer the end; in fact, if this communication gives birth to any idea in any other person, so that the end can be accomplished; this is all I have in view. I think, sir, that at times like the present, when all the nations of the world by turns are our enemies, it is the duty of every Englishman, to endeavour to discover, if it be possible, among ourselves, resources to satisfy our wants. Why should we, if it be possible to avoid it, be dependant on any nation? We ought to endeavour to bring into the greatest degree of usefulness every article our own country produces. By so doing, we are gaining more real wealth, than by the fluctuations and hazards of commerce. Mahogany in the last six or seven years has nearly doubled in price, and this is still increasing: and when, as is the case, every man of the middle class is imitating the luxury of the higher orders; not only imitating them in their extravagance and expensive mode of living, but in his furniture, almost every article of which in his house must be mahogany; this increases the demand to a great degree, and in proportion also the price: so that a large portion of the wealth of this country goes in purchasing in a foreign market this expensive article of luxury. If it were a native of our own soil, the evil would not be so great; for at all events the money would remain in the country, and in time would find its way again into circulation.

We should endeavour to render ourselves independent of foreign nations.

Mahogany greatly risen in price, and much used.

Iron proposed for bedsteads.

Admits of much elegance and lightness.

The substitute I mean to propose is iron. In bedsteads, for instance, the posts or pillars, as well as the frame itself, might be cast hollow, beautifully wreathed up the posts with flowers, festoons, or clusters of fruit, or embossed with numberless fanciful ornaments, which the workman might touch up with his graver and chissel, to clear the foliage, &c. from the sand, and to make the flowers sharp and neat before they go to the finisher. The painter might colour them so as to have a more elegant and more handsome appearance than it is possible to give to carved wood: and besides they might be cast so light, and in such chaste symetry, as cannot be accomplished in wood. This would give employment to many of our manufacturers, as every Japanner could

could employ his hands in painting and polishing them; there would be ample scope for ingenuity in the cornices, and in the ornamenting and finishing them; and I think they might be sold at a considerably less price, than the carved mahogany ones are now made at.

Not expensive.

Also chests of drawers, bookcases, and bureaus, might all be made in sheet iron. The frames and mouldings might be rolled in rollers with grooves, with all kinds of patterns indented or engraved in the rollers, such as foliage, plain, fluted, or beaded stripes, or any ornamental work. The mouldings would thus be made rapidly in the extreme. The pannels might be cut out to fit the article, the fancy of the workman had made, in sheet iron. The mouldings, framing, and pannels, might then be beautifully japanned, painted, and polished, either to imitate mahogany; or with red, black, or any kind of coloured grounds; the pannels painted with landscapes, flowers, fruits, animals, or any device fancy might dictate; the mouldings might be made in all kinds of Gothic or other shapes and forms, and the pannels fitted to them, and the whole piece of furniture screwed together when completely finished. The drawers might be made with light iron framing; filled up with wire work, which would make them very light; and afterward lined with silk, cloth, paper, or any substance most convenient. This would diminish the consumption of the cheaper wood, used for the drawers, &c.

For chests of drawers and bookcases.

I do not think a piece of furniture finished in this manner would be any heavier than one made with wood. For the sheet iron for the pannels need only be of sufficient thickness, to stand to its form without bending, and the framing of proper strength to hold firmly together. Furniture made in this manner would certainly be more beautiful, and if an accident of fire were to happen, the property contained in it would be saved. The article would only want fresh japanning and paintihg again, if the flame had destroyed its beauty.

Such furniture not heavier than wood.

More beautiful, and a security against fire.

Large pieces of furniture, when required to be removed, might easily be taken to pieces, as all parts would be screwed together and put up again at a trifling expense, without the least injury: while at present a large, handsome, and valuable

Convenient for removal.

valuable piece of furniture, in removal, either by the carelessness of the people employed, or its unmanageable size, is almost sure to be very much injured.

Other advantages.

Indeed there is great scope here for ingenuity and improvement; it would give employ to vast numbers of men; it would consume our own produce, that is iron; we should get an article that would endure for ages; and we should in part be independent of any other country, for the material that forms a beautiful and useful ornament in our houses.

Particularly for libraries.

Consider the advantage a scarce and valuable library, fitted up with light iron shelves and doors, would have in case of fire. The iron pannels, as well as the doors, would always fit tight, and never warp, as wood does; and if enveloped in flame, being almost if not quite air tight, it would be next to impossible, that the books or valuable manuscripts should be burnt, or so destroyed, let the fire be ever so intense, as to be lost. They might be blackened, and in part reduced to a state almost like that of the papiri at Herculaneum, were the fire to continue a great length of time without interruption; but they would not be entirely lost; and labour and patience might restore them to the world. Those valuable articles of antiquity, or indeed all valuable documents, that in their wooden cases are ever in danger of being lost to the world by fire, would be secure if preserved in iron. Modern publications, indeed, can always be restored to the sufferer, at a price; but to save those, that would be for ever lost to society, those that no money could purchase, no power on Earth could restore, is surely an object ardently to be desired.

Doors.

Doors for halls, doors of all kinds, with light iron frames, and neatly pannelled, would be neither heavier, nor dearer, I think, than those now in use; and if they should be a little heavier, custom would soon reconcile us to the use of them. In case of fire, an iron door might perhaps save the contents of a valuable room; instead of serving, as doors now do, to conduct the devouring element to the next apartment. Drawing-room doors, especially, and various other articles, if expense were no object, might be made of more beautiful and delicate workmanship, than it is possible to produce in wood. For instance, all kinds of Gothic scrolls, might be made

made of iron, light and elegant, and every plate or pannel, that fitted them, might be painted with any kind of device imagination and taste might devise, beautiful as the ancient painted windows of cathedrals, and they would endure for centuries.

Conclusion.

But I need not farther expatiate on this subject. If from the above hints, any one would enter upon it with that kind of spirit the thing requires, select ingenious mechanics, and study to introduce it with lightness, elegance, and taste; when it is considered, that the price in general would be less than mahogany, that it would be handsomer and more durable, that it is the production of our own nation, and that it would give employment to vast numbers of our own countrymen; I flatter myself with the idea, that in a great many instances, it would be adopted.

I am, Sir,

Birmingham, Your obedient humble servant,

Caroline Street, March 16, 1809. B. COOK.

XI.

On ascertaining Square Numbers and Biquadrates by Inspection. By W. Saint, *Esq.*

To Mr. NICHOLSON.

SIR,

Woolwich, March 15*th*, 1809.

To ascertain by inspection whether a number be a perfect square would often save much trouble.

Having frequently experienced, in the solution of questions involving a quadratic equation, and more particularly in such as relate to the diophantine algebra, a considerable degree of inconvenience and trouble from not being able to ascertain whether a number be a perfect square or not, without the tedious operation of extracting its root; I have thought, that the following rules or propositions, which I have accompanied with their demonstrations, might prove useful to many of your readers, by enabling them, on inspection, to ascertain a great variety of forms of numbers,

which

which can never be squares, and thus at least preventing them the trouble of perhaps many useless extractions. Should you, Sir, deem these propositions of sufficient importance to occupy a place in your useful miscellany, your insertion of them will oblige,

Sir, your very humble servant,

W. SAINT.

A square number cannot terminate with 2, 3, 7, or 8:

Proposition 1.—A square number cannot terminate with 2, 3, 7, or 8.

Demonstration.—The terminating figure of every product arises from the multiplication of the terminating figures of its factors. The terminating figure therefore of every square number must arise from the product of 0×0, 1×1, 2×2, 3×3, 4×4, 5×5, 6×6, 7×7, 8×8, 9×9; and these products it is evident, can only end with 0, 1, 4, 9, 6, and 5, and never therefore with 2, 3, 7, or 8. Q. E. D.

or with an odd number of noughts.

Prop. 2. A square number cannot terminate with an odd number of ciphers.

Demonstration. Since every square number ending with 0 must have its root ending with 0, such a root must be of the form $10\,m$, and the square number itself therefore of the form $100\,m^2$; where it is evident, that whatever value be given to m, the product $100 \times m^2$ must terminate with two ciphers. It is also obvious, that it cannot end with more than two, unless m^2 end with an 0; which again can only be when m is of the form $10\,n$, or m^2 of the form $100\,n^2$, and therefore $100 \times m^2$ of the form $10000 \times n^2$, which must end in at least four ciphers; and so on as far as we please. Q. E. D.

If it terminate with 4, the last figure but one must be even.

Prop. 3. If a square number terminate with 4, the last figure but one will be an even number.

Demonstration. For such a square number must have its root ending in 2, or 8; this root will therefore be of the form $10\,m \pm 2$, and its square of the form $100\,m^2 \pm 40\,m + 4$; where, whatever value be given to m, the sum or difference of the first and second terms will give an even number of tens, and an even number of tens plus 4 must have the last figure but one an even number.

Prop.

Prop. 4. If a square number terminate with 5 it will terminate with 25.

If with 5 the preceding figure must be a 2.

Demonstration. For such a square number would have its root ending in 5, that is, would have its root of the form $10m + 5$, and consequently the square number itself would be of the form $100m^2 + 100m + 25$; where it is evident, that, whatever value be given to m, the sum of the two first terms will end with two ciphers, and therefore that the whole sum will terminate with 25. Q. E. D.

Prop. 5. If a square number terminate in an odd number, the last figure but one will be an even number, but if it terminate in any even number, except 4, the last figure but one will be an odd number.

If with an odd number, the last figure but one will be even: if with 6, it will be odd.

Demonstration. By prop. 1, if a square number end in an odd number, it must be in 1, 5, or 9; and by the last prop. when it ends in 5, the last figure but one will always be 2, which is an even number; and when it ends in 1, or 9, its root must end in 1 or 3, that is, must be of the form $10m + 1$, or $10m + 3$; and therefore, the square number itself of the form $100m^2 + 20m + 1$, or $100m^2 + 60m + 9$; where, whatever be the value of m, it is evident, that the sum of the two first terms in either expression will give an even number of tens; and an even number of tens plus 1 or 9 will have the last figure but one an even number. Again, if a square number end in an even number except 4, by prop. 1 it can only be in 6, and its root must end in 4 or 6; that is, it must be of the form $10m \pm 4$, and consequently the square number itself of the form $100m^2 \pm 80m + 16$; where it is evident, that, whatever be the value of m, the sum or difference of the two first terms will always give an even number of tens, and an even number of tens plus 16 must have the last figure but one an odd number. Q. E. D.

Corollary. Hence no square number can terminate with two equal figures, except two fours or two ciphers.

A square number cannot terminate with two similar figures, unless 4s or 0s, and not with more than three 4s.

Vide cor. to prop. 6.

Prop. 6. A square number cannot terminate with more than three fours.

Demonstration. For, if it could end in four fours, such a number might be expressed by $a \cdot 10^4 + 4 \cdot 10^3 + 4 \cdot 10^2$

+ 4

$+ 4 . 10 + 4$; and being a square number, it would be such when divided by 4— that is to say $25a . 10^2 + 10^3 + 10^2 + 10 + 1$, or its equal $(25a + 10) . 10^2 + 10^2 + 10 + 1$, or $(25a + 11) . 10^2 + 10 + 1$, would be a square number. Now it is evident, that $(25a + 11) . 10^2$, whatever value be given to a, would end with two ciphers; and therefore, that the whole expression would terminate in two ones, which is impossible by cor. to prop. 5. Q. E. D.

No square can consist of equal digits.

Cor. Hence no square number can be contained under any number of equal digits.

Cubes.

Scholium. If this speculation be extended to cube numbers, it will be found, that such numbers may terminate with any of the nine digits, and that it is not therefore so easy to ascertain whether a number be a perfect cube from the nature of its terminating figure.

Biquadrates.

With regard to biquadrates, or fourth powers, we may observe, that, as these are square numbers also, whatever has been demonstrated above relating to square numbers, excepting prop. 1, holds equally true for biquadrates. We may farther remark, that biquadrates terminate in 0, 1, 5, or 6; and that, when they terminate in 0 or 5, their roots will terminate with 0 or 5 also: moreover, that when the root terminates in 5, the biquadrate will terminate in 625.

XII.

Some Account of Cretinism. By HENRY REEVE, *M. D. of Norwich. Communicated by* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.**

Cretinism, a species of mental imbecility, endemic in some parts of Switzerland.

FELIX PLATER, in one of his observations, gives the history of a species of mental imbecility, which he saw in passing through the village of Bremis in the Valais. Cretinism, a word of uncertain derivation, is the name employed by the inhabitants of Switzerland to denote this disease, which is endemial in several districts of that country.

* Philos. Trans. for 1808, p. 111.

It

It had probably existed long in those parts; for Plater mentions cretins as being very common both in the Valais and in Carinthia, but the peculiar marks of these wretched beings were not generally known before he described them*. Mons. de Saussure has furnished the most minute and accurate account both of the appearances of the disorder, and of the circumstances which seem to produce it; and Mr. Coxe and several travellers have noticed the symptoms of cretinism, without adducing any satisfactory explanation of the causes to which it may be ascribed. Malacarni of Turin and Professor Ackermann have given a very accurate description of several cretins that they dissected; and beside some detached essays by different authors, a very full account of this malady is to be found in an "*Essai sur le Goître et Crétinisme par M. Fodere,*" published at Paris in 1800.

Usually connected with bronchocele.

My curiosity led me some time ago to inquire more particularly into the nature and causes of cretinism, because it is usually connected with goître, or bronchocele; I was indeed led to this inquiry, partly by the hope of discovering some function for the thyroid gland, more satisfactory than what is commonly alleged; but in these expectations I have been disappointed.

Objects of inquiry.

In the summer of 1805, I had an opportunity of seeing several cretins at Martigny and Sion, and other villages in the Valais; and I was glad to compare what had been written upon that subject, with what my own observation could suggest. By inquiries on the spot, I intended to learn what connection subsisted between weakness of the intellectual faculties and the swelling of the thyroid gland: what were the moral and physical circumstances, which could influence the condition of the inhabitants, so as to make idiocy so prevalent; and what were the most efficient modes of relief. The following results I beg leave to lay before the Royal Society.

Cretinism common to mountainous countries.

Cretinism is found not only in the vallies of the Alps, both on the French and Italian side of these mountains, but in the mountainous parts of Germany and Spain; and it was

* F. PLATERI *Praxeos Medica, Cap.* III. *Basil.* 1656.

observed

observed in Chinese Tartary by Sir George Staunton, in a part of that country much resembling Switzerland and Savoy in its alpine appearance. The enlargement of the thyroid gland, called goître, is the most striking feature in the unsightly aspect of a cretin; but this is not a constant attendant. His head also is deformed, his stature diminutive, his complexion sickly, his countenance vacant and destitute of meaning, his lips and eye-lids coarse and prominent, his skin wrinkled and pendulous, his muscles loose and flabby. The qualities of his mind correspond to the deranged state of the body which it inhabits; and cretinism prevails in all the intermediate degrees, from excessive stupidity to complete fatuity.

Appearance of cretins.

Four examined.

At a small village, not far distant from Martigny, I examined four cretins. One lad, twelve years old, could speak a few words; he was of a weak and feeble frame, silly, but had no goître. Another boy, nine years old, was deaf and dumb, idiotic, with no goître, the only child of his mother, who has a large goître which affects her respiration and her voice, though in other respects she is intelligent and well formed, and the father enjoys good health; they are not natives of this place. I saw a family in which all the children were cretins; the eldest died a year ago, a miserable object; the second, a girl, twelve years old, is deaf, and dumb, and cross eyed, and has a monstrous goître, with just intelligence enough to comprehend a few natural signs; the third is a boy six years old, small and feeble, abdomen enlarged, no goître, very feeble in mind and body, not entirely deficient in understauding; the mother had a moderate sized goître, but was quite free from any mental affection; the father neither goîtrous nor stupid, but of a delicate constitution.

Cretinism and bronchocele not necessarily connected.

There is no necessary connection between goître and cretinism, notwithstanding the assertions and ingenious reasoning adduced by Fodere. It is probable, the one has been assumed as the cause of the other, from the enlargement of the thyroid gland being a frequent occurrence in cretins; and as it forcibly strikes the observer from the deformity it occasions, this strong impression may have converted an accidental, though frequent occurrence, into a general and necessary

necessary cause. Cretinism is frequently observed without any affection of the thyroid gland, and that gland is often very much enlarged without any affection of the intellectual faculties. There seems to be some similarity between cretinism and rickets, as they both take place in infancy, are both characterized by feebleness of body, and sooner or later by feebleness of mind, and they both affect males and females equally; but there is no sort of connection between persons afflicted with bronchocele in England, and with rickets. For although it might be granted, that there is some delicacy of frame in females about the period of pubescence when bronchocele usually occurs, yet neither irregular formation of the bones, nor weakness of the intellectual powers, are common symptoms attending bronchocele in Britain. Some similarity between cretinism & rickets. Bronchocele.

To what peculiarities then, in the physical constitution of certain districts, are we to ascribe the production of this singular malady? Saussure's description of the Valais is exceedingly precise and accurate, and the causes which he has alleged appear sufficient to account for the phenomena. The vallies where cretinism is most frequent, are surrounded by very high mountains; they are sheltered from the currents of air, and exposed to the direct and still more to the reflected rays of the sun. The effluvia from the marshes are very strong, and the atmosphere humid, close, and oppressive. All the cretins that I saw were in adjoining houses, in the little village called la Batia, situate in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situate higher up the mountains, no cretins are to be seen, and the mother of one of the children told me, of her own accord, without my asking the question, that her child was quite a different being when he was up in the mountain, as she called it, for a few days. What are the causes of cretinism? Situation,

The production of cretinism, by the bad quality of the air and the food, the neglect of moral education, and other evils attendant upon poverty, is supported by facts so pointed, that the greater number of cases in mountainous districts where snow water abounds, may safely be ascribed and poverty.

Not drinking snow water,

to these general causes. The notion of snow water being the cause of goître, and consequently of cretinism, seems to have been derived from Pliny (Lib. II, cap. 37,) and copied by almost every succeeding writer, because it coincides with their hypotheses of cold and crude matters, although directly contradicted by facts. In the first place, persons born in places contiguous to the glaciers, who drink no other water than what flows from the melting of ice and snow, are not subject to this disorder; and secondly, the disorder is observed in places where snow is unknown.

water from melted ice,

or calcareous water.

The theory of water impregnated with calcareous matter being the cause is equally unfounded; because the common waters of Switzerland excel those of every other country in Europe for purity and flavour. There is not a village, or a valley, but what is enlivened by limpid rivulets or streams gushing from the rocks. The water usually drunk at la Batia and Martigny is from the river Dranse, which flows from the glacier of Saint Bernard, and falls into the Rhone; it is remarkably free from earthy matter, and well tasted. At Martigny there are two or three pumps, the water of which is pure and equally fit for culinary purposes, but said to be unwholesome, without any good reason. At Bern, the water is extremely pure, yet, as Haller remarks, swellings of the throat are not uncommon in both sexes, although cretinism is rare. With regard to the alleged causes of goître, the general opinion of its being endemial in mountainous countries is of no value, because the disease is rare in Scotland, and very common in the county of Norfolk.

Bronchocele not peculiar to mountains.

Progress of cretinism.

The causes of cretinism begin to operate upon the system soon after, perhaps even before birth; the want of energy in the parent is communicated to the offspring; the children become deformed and cachectic very early in life, the growth and developement of the body is impeded, the abdomen becomes enlarged, and the glands swelled in various degrees; and the powers of the mind remain dormant, or become entirely obliterated, partly from want of proper organization, and partly from total neglect of every thing like education.

It

It might be expected, that the dissection of cretins would throw some light upon the series of phenomena associated together in the origin and progress of this singular affection; but the people are so superstitious, that it is very difficult to procure bodies for anatomical examination. However, some dissections have been made, and the appearances in the cranium are very curious. From the description of a cretin's skull by Ackermann, it appears, that the cavity for the reception of the pons Varolii and medulla oblongata was completely obliterated, and that in which the cerebellum is lodged so much diminished, that it scarcely exceeded one third of its natural capacity. The return of the venous blood must have been considerably impeded by the maleconformation of the foramina. Appearances nearly similar were observed by Malacarni and by Fodere.

Little light derived from dissection.

The skull much affected.

In the anatomical museum at Vienna, I saw a cretin's skull, from which Professor Prochaska was so obliging as to permit me to have two drawings taken. It is the cranium of a cretin, who died at the age of thirty, yet the fontanelle is not closed, the second set of teeth are not out of their sockets, and none of the bones are distinctly and completely formed. The head is very large, the face small; it is like the skull of an adult joined to the face of a child; every part bears marks of irregularity in the growth and formation; and irregular action must have been the concomitant of such a morbid structure, whether the appearances be considered as cause or effect.

Description of a skull of a cretin thirty years old.

The four angles of the os malæ are not well defined; the zygomatic and maxillary processes of this bone are wanting; the nasal processes of the superior maxillary bone are very large, and exhibit no marks of union with the os malæ; the ossa nasi are very small; the temporal bone is imperfectly formed; the zygomatic process terminates at the coronoid process of the lower jaw; the mastoid and styloid processes are wanting, and the pars petrosa remarkably small; the squamous portion not distinctly marked; the os occipitis unusually large, and numerous additional bones, ossa triquetra, along the whole course of the lambdoid suture. The other deviations of the natural structure

cor-

corresponded with those already described by different writers.

Proof of the action of physical causes on the mind.

There is no fact in the natural history of man, that affords an argument so direct and so impressive, in proof of the influence of physical causes on the mind, as cretinism. It shows moreover, that the growth of every part is essentially connected with the conditions in which it is fit to exercise its peculiar functions; and in this respect, it fares with the intellectual as with the bodily powers.

Cretinism may be prevented.

The most decisive argument in proof of this opinion is, that cretinism may be prevented by removing children from the confined and dirty places where it prevails, and nursing and educating them in the higher parts of the mountains. Within these last ten years, the number of cretins has diminished, the condition of the lowest class of society is somewhat bettered, and more attention is paid towards that diseased constitution which is the forerunner of mental imbecility. I did not find that the poor creatures took any pride in having any of their children ideots, or *bien heureux*, as some authors assert; on the contrary, the parents were very much ashamed of acknowledging, that any cretins belonged to their families; and it was after repeated attempts, only by declaring myself to be a physician, that I could get access into their houses to examine any of those wretched beings in the human form. The burnt sponge is known as a remedy for the goître among the people where it is most prevalent; but it is seldom administered, because the disease is so common, that it does not attract notice, nor affect in general the ordinary functions of life. And as to cretinism, this seems to be looked upon as belonging to indigence and poverty; for in every place where I saw cretins, many well looking persons of both sexes resided, and these were, without exception, persons of a higher class in society, who lived in better houses, and could supply both their moral and physical necessities.

Confined to the poor.

Analogy between cretinism and rickets.

I might perhaps have insisted more upon the analogy between cretinism and rickets, for there is a remarkable coincidence in the literary history of these two diseases, as well as in many other points. Glisson first described rickets, as

as this disease appeared in this country, in the middle of the 17th century, about the same time that Plater mentions cretinism. The origin of both names is equally obscure; and since some of the remote causes are now discovered, it is to be hoped the diseases themselves will gradually disappear, and in some happier age be known only by description.

XIII.

On the Composition of the Salts of Barytes. By Mr. ARTHUR AIKIN.

To Mr. NICHOLSON.

SIR,

THE ascertainment of the exact composition of the salts of barytes is of such essential importance to the accuracy of chemical analysis, that I shall make no apology for troubling the readers of your very respectable Journal, with the following details of a few experiments instituted for this purpose. Composition of barytic salts important.

The salt selected by me as the basis of these experiments was the muriate of barytes, on account of its ready solubility in water, and its inalterability at a red heat. I shall begin therefore by describing the precautions, that I took to ensure the perfect purity of the substance on which I was operating. Muriate selected for experiment.

SECT. 1. *Preparation of pure Muriate of Barytes.*

A quantity of crystallized native carbonate of barytes was digested in cold and dilute muriatic acid, till all effervescence had ceased, a portion of the carbonate remaining undissolved; the solution was then filtered and crystallized by rapid evaporation. The salt thus procured was ignited in a platina crucible, and became of a light ochrey yellow from the muriate of iron, that was decomposed. It was then dissolved in cold water and filtered, by which the oxide of iron was separated, and the liquor came through quite Precautions to ensure its perfect purity.

quite limpid and colourless. The solution, being evaporated to a pellicle, was decomposed by rectified alcohol, which threw down the muriate of barytes, and retained in solution the small portion of muriate of strontian, and any other earthy muriate that might have been casually present. The precipitate, being well washed in alcohol, was afterward ignited; but being in some degree fouled by a small portion of charcoal from the decomposition of the alcohol, it was redissolved in water, filtered and evaporated to dryness, and then ignited. There was thus obtained a salt of a pure white colour, which dissolved in cold water without leaving any residue, and which I consider as pure muriate of barytes.

SECT. 2. *Proportion of Water in crystallized Muriate of Barytes.*

Proportion of water in the crystals,

A quantity of the above muriate was dissolved in warm water, and left to crystallize by spontaneous evaporation. The salt obtained, after being dried by an exposure for several days to the air, weighed 183·25 grs. It was then fully ignited for about an hour, at a heat somewhat less than that required for its fusion, and lost in weight 26·75 grs., which I conclude to be only water, as the residue was perfectly soluble in cold water.

In another experiment 100 grs. of crystallized muriate, that had been dried by the heat of boiling water, were reduced to 85·5 grs. by a heat somewhat inferior to ignition; being then heated to a low red it weighed as before 85·5 grs.; it was then kept in fusion for about a quarter of an hour, by which it lost less than 0·25 gr.

14·5 or 14·6 per cent.

Hence the water of crystallization in muriate of barytes amounts to between 14·5 and 14·6 per cent.

SECT. 3. *Ratio between the Muriate and Carbonate of Barytes.*

Ratio between the muriate & carbonate.

100 grs. of ignited muriate were dissolved in water, and decomposed by carbonate of soda. The precipitate, when edulcorated and dried at a heat superior to that of boiling water, weighed 93 grs.

156·5

156·5 grs. of ignited muriate were decomposed in the same manner, and afforded 146·75 grs. of carbonate, or 93·77 per cent.

Hence (averaging the results of the two experiments) 100 grs. of ignited muriate afford 93·38 of carbonate: and 100 grs. of carbonate contain the same quantity of barytes as 106·6 of ignited muriate.

SECT. 4. *Composition of Carbonate and Muriate of Barytes.*

93 grs. of carbonate, dried at nearly a red heat, lost 20·5 grs. of carbonic acid by solution in muriatic acid; or 22·04 per cent. Composition of

145·5 grs. of carbonate, by similar treatment, lost 31 grs. of carbonic acid, or 21·3 per cent.

Hence, on an average, carbonate of barytes consists of the carbonate,

21·67	carbonic acid
78·33	barytes
100.	

Ignited muriate of barytes consists of ignited muriate,

73·14	barytes
26·86	muriatic acid
100.	

And fully crystallized muriate contains crystallized muriate,

62·47	barytes
22·93	muriatic acid
14·6	water
100.	

Hence also, 100 of carbonated barytes contain the same quantity of earth as 124·9 of the crystallized muriate.

SECT. 5. *Composition of Sulphate of Barytes.*

100 grs. of ignited muriate of barytes were added by degrees to some very pure sulphuric acid in a platina crucible. When the effervescence had subsided, the mixture was gently heated, and the excess of acid was saturated by carbonate

bonate of ammonia. The whole was evaporated to dryness, and then kept at a full red heat for a considerable time after all visible fumes had ceased. The sulphate of barytes thus produced weighed 110·75 grs.

and sulphate of barytes.

Then as 100 grs. of ignited muriate contain 73·14 barytes, 100 parts of sulphate of barytes contain

66·04 barytes
33·96 sulphuric acid

100.

Composition according to Klaproth.

The results of the above experiments nearly correspond with those of Klaproth, according to whom

Carbonate of barytes consists of { 21·8 carbonic acid, 78·2 barytes.

Sulphate of barytes .... { 33·55 sulphuric acid, 66·55 barytes.

and 100 parts of carbonated barytes yield 121·04 of crystallized muriate.

ARTHUR AIKIN.

XIV.

A Memoria Technica for double Elective Attractions. By THOMAS YOUNG, *M.D. F.R.S.*

To Mr. NICHOLSON.

SIR,

Table of double elective attractions.

I Have inserted, at the end of the Syllabus of my Lectures on the Elements of the Medical Sciences, a short table, containing the results of 1260 cases of double elective attractions. For the convenience of those who may wish to retain these results in memory, I have since employed a few leisure moments in expressing the table in the form of technical hexameters. I must beg leave to refer, for an explanation of the principles on which the arrangement is founded, as well as for a few particular doubts and exceptions, to a paper which will appear in the Philosophical Transactions: and I shall at present only observe, that each line expresses a column of the table; and that when four substances, which stand in any column, are mixed, they will

will arrange themselves in such a manner, that the bases will always be united to the respective acids which stand nearest to them.

I am, Sir,

Welbeck Street, Your very obedient servant,
22 *March,* 1809. THOMAS YOUNG.

CONTENTIO AQUATICA; VICTORIA; REQUIES.

REBARIsne modo posse adfore bellica ROSTRA?
Des nautam satis apta cibo refovere ALIMenta;
Cor superest sanum; flabitque OPTatus abunDE
Spiritus; has animi ira feret tibi acerriMA GAZas.
AST BRONTes animosus acerbo fœdere paLMas
Cæsus fert; ut pro rebus monet APTA SODales!
Si possit, fato tubicen meMOR addat honores.
Postulat ossa relata, heu! flebite condere mARMOR.
Spes est fixa, bonum cœli GAZis fruiturum.
ALMA huic pax fiat orbi, lassis omniPOTEns DEs
O pater! Ut flebo jussus canere armigenûM vim!
Dire opifex belli, cesses normAM abjicere omnem.
Pax fessos bona mulcet, GAZAS lætior auri.
PRæSUMAM GAZas nempe adfere rursus ab alto huc;
MIra dabit lucra pax, fortassis in ultimA MUndi.

XV.

An Abstract of a Meteorological Journal for the Years 1807 *and* 1808, *kept at Middleshaw near Kendal, in Latitude* 54° 20′. *By* JOHN GOUGH, *Esq.*

MY object in communicating the following table to the Philosophical Journal is, to turn the attention of meteorologists to two points of their favourite science, which perhaps have been too superficially examined. One of these points is the diurnal variation of temperature at different times of the year. The object of the communication.

This is a subject of which we have a very imperfect knowledge; and the reason is obvious: for the variation in question is not easily determined by the common thermometer, at least in summer, because the sun commonly

monly rises before the observer, and the morning observation is registered at too late an hour. To obviate this inconvenience, I made use of the thermometer invented by Mr. Six; which, with a little assistance from the observer, gives a correct account of all its variations, by noting each day the two extremes of its range.

Six's thermometer recommended.

Diurnal variation of temperature.

The diurnal variation of temperature determined by this instrument, is certainly more correct than the results of a common thermometer. As for the utility of the inquiry, I have only to observe, that meteorology is at present in its infancy; and the cultivators of the science have little to do but to collect facts for the use of their successors.

Tables of the kind here recommended, formed in different situations and latitudes, will perhaps prove necessary, when the materials of a rational theory have been collected from long observation. In the mean time we know for a certainty, that the phenomenon in question arises from the sun's annual motion between the tropics: but the influence of this luminary is disturbed in the regularity of its effects by the vicissitudes of the weather; for the variation is greatest when the atmosphere is clear, and least at the same season when the air is obscured with clouds and rain. These are manifestly causes of irregularity; but they have not sufficient power to prevent the necessary consequences of the sun's motion in the ecliptic, they only retard or accelerate his effects. For supposing the Earth to be destitute of an atmosphere, the sun would produce two maxima and two minima of variation at stated times in the course of a year, according to the doctrine of radiant heat. The minima would coincide with the coldest and hottest days, and the maxima would happen after the two equinoxes, perhaps about the times of mean temperature. These extremes appear in the table, but they are not periodical; which is also the case with the hottest and coldest days, as well as the seasons of mean temperature.

Rain collected at different elevations.

The second subject in meteorology, to which I was desirous to attend, was the comparison of the rain collected at different elevations, in the same neighbourhood. For this purpose, I made use of two gauges constructed exactly alike; one of them stood in a garden in the bottom of a valley,

valley, and the other was placed on the top of a hill almost directly north of the former, and 91 yards above it; a right line joining the two stations cannot exceed 500 yards. Negligence in one instance, and an accident in another, interrupted the series of observations in the first year, but the latter is complete; and I think the whole goes to prove the results of the two gauges to differ less in summer, than they do in winter.

As for the table, no part of it requires to be explained, except the fifth column, marked Ratio. To form this, the numbers in the third column are multiplied by 1000, and divided by the corresponding numbers in the second; so that the lower exreme of the monthly mean range is invariably denoted by 1000, and the higher by the number, which stands in the fifth column opposite to any particular month. Table explained.

Month. 1807.	HEIGHT of THERMOMETER.				Rain in inches.	
	Least.	Great.	Mean.	Ratio.	Low. G.	Upp. G.
January	31·45	40·06	35·75	1273	2·799	2·517
February	32·67	40·71	36·69	1246	4·768	1·468
March	30·29	40·93	35·61	1351	1·798	0·132
April	37·46	49·56	43·51	1323	2·839	1·804
May	44·82	58·83	51·82	1312	4·552	3·262
June	48·75	63·28	56·01	1298	2·404	2·031
July	55·11	69·14	62·12	1254	5·310	2·716
August	53·19	66·91	60·05	1257	4·219	3·214
September	40·90	54·16	47·53	1324	3·014	2·340
October	46·17	53·70	49·93	1163	6·382	5·1[illegible]0
November	31·18	38·45	34·81	1233	3·625	2·914
December	30·76	36·93	33·84	1200	2·246	1·564
Annual Means.	40·229	51·055	45·642	1269	Total of Rain. 38·631	29·142

Jan. 1808.	33·06	35·87	34·31	1085	4·725	2·517
February	31·16	39·76	35·46	1275	2·262	1·468
March	32·43	42·43	37·43	1308	0·160	0·122
April	34·51	46·71	40·61	1353	2·542	1·804
May	47·53	62·33	54·93	1311	3·692	3·262
June	50·98	64·06	57·97	1256	2·376	2·031
July	55·60	71·52	63·56	1286	3·388	2·716
August	54·36	66·79	60·57	1228	4·219	3·214
September	48·65	59·75	54·20	1228	3·014	2·340
October	38·30	47·93	43·11	1251	6·382	5·190
November	37·58	40·98	39·28	1090	3·625	2·914
December	33·09	38·22	35·65	1155	2·246	1·564
Annual Means.	42·10	51·36	46·73	1236	Total. 38·631	29·142

XVI.

*An Essay on Electrical Attractions and Repulsions; by Mr. *** *.*

Two magnetic fluids questioned.

MY experiments on magnetism, which give this property to several needles in opposite directions by a single electrical shock, led me to doubt the existence of two fluids, which, according to the theory of Mr. Coulomb, repel each other in every section of a magnet. From this doubt arose a second. I said to myself, do electrical repulsions exist without previous attraction? in other words, are there two electrical fluids, possessing with respect to each other the

Are there two electrical fluids repelling each other?

* Journal de Physique, vol. LXIII, p. 378.

repulsive

repulsive power of Newton *? Accordingly I took up the most modern treatise on electricity, and at the article "Of Repulsions and Attractions," I found six experiments, which are adduced to prove, that this attraction and repulsion are alternate. I believe, that a double affinity naturally explains this alternate movement; and I am persuaded, that the more we simplify our theories, to account for natural phenomena, the nearer we approach the truth.

1st experiment to prove alternate attractions and repulsions.

1*st experiment* of Mr. Libes, on attractions and repulsions, in his new Dictionary of Natural Philosophy, vol. I. p. 351.

"Rub with the hand a glass tube, so as to render it perceptibly electrical. Let fall on this tube a bit of foil, down, or any other light body; it will be attracted, and suddenly repelled by the tube. If in this last state of repulsion we follow it with the tube, it will fly off with rapidity in a given direction; but if it meet in its course with another conducting body, that is not electrified, it returns immediately to the tube, and afterward suddenly separates from it; so that if it hung freely suspended by a silk thread between the tube and the foreign body just mentioned, it would fly alternately from one to the other."

Accounted for by attraction only.

It is known, that metal has a considerable affinity for the electric matter: and it is this affinity, that attracts it toward the tube, the latent electricity of which is not merely excited by the friction, but probably some has been attracted from my body to its surface by the same cause; it being drawn toward it, to acquire as much as it can of this excited fluid. If, while it is in this state, there be any substance near, that has likewise an attraction for the same fluid, and this affinity be sufficiently strong to overcome the gravitation of the foil, it will attract it, seize in its turn the electric fluid, and convey it to the ground, which is equally greedy of it. If the intensity of the foil be still sufficient, to carry it anew toward the tube, these movements will continue to alternate, till the attraction of gravitation exceeds that of electricity. If there be no con-

* Optics, quest. 31: or Theory of the Earth by Mr. Delamétherie, vol. III. par. 614.

ducting

ducting surface to attract the foil, it will be attracted by the moisture of the air. The reflections I shall make on the substance of glass to explain the 4th Experiment will serve to account for the down, which is a nonconductor, acquiring in this instance an affinity for the fluid.

I do not perceive here any repulsion: for, according to the Author himself, the metallic leaf goes to part with its electricity to the conducting substance. In the electrometer with pith balls, that diverge from each other, by which the electrical repulsion is frequently attempted to be proved, these balls separate only because the aqueous vapours of the air attract them. This is the reason of the movement of the little pendulum of Henley's electrometer, which I find to be one of the best. In fine, the divergence of all electrometers, as it appears to me, is produced by the attraction of aqueous vapours, or the action of some conducting substance, which attracts leaves and small balls saturated with electric fluid.

2d Experiment.

2d experiment.

"Suspend freely to the conductor of an electrical machine a fringe of thread twisted together so as to form a tuft; and the moment you electrify the apparatus, you will see all the threads, that were united together, separate from each other, and this to a greater distance in proportion as the electricity is powerful."

No repulsion.

Here the air, as with respect to the electrometer, attracts and separates as far as possible the threads. Every natural philosopher no doubt will agree, that the vast evaporation of water, that takes place from the surface of the globe, must impregnate the atmosphere more or less with aqueous vapour, even in what we call dry weather, and that this imparts to it a great affinity or attraction for the electric fluid. The circumambient air around the electrical machine, therefore, is that which can most readily effect its union with this fluid. That which is farther off, having the same tendency, must consequently attract toward it as far as possible every substance saturated with it. Hence it follows, that, the more of the fluid every thread has imbibed, the more

more will the aqueous attraction act on it, and to the greater distance.

3d Experiment

"On a plate of metal five or six inches in diameter put some shreds of gold leaf, and two inches above them let a similar plate of metal be suspended from the conductor, so as to be electrified by it. These little bits of gold leaf will be immediately attracted by the upper plate, and afterward suddenly repelled to the lower, so that these attractions and *repulsions* will continue as long as the conductor remains electrified. 3d experiment.

"To render this experiment the more pleasing," continues our author, "we may substitute for these bits of gold leaf little painted figures, which, alternately attracted and *repelled* by the upper plate, will appear to dance between the two."

This phenomenon, as the preceding experiments, is to be explained by the double play of affinities. These bits of gold leaf, or little figures, are alternately attracted toward the upper plate in consequence of their affinity for the electric fluid, and toward the lower, which has a communication with the ground, and consequently a reciprocal affinity for the fluid, till the conductor no longer contains enough of the fluid to overcome their weight. Lastly, take a fine needle with two points, that can move freely between the two plates, it will raise itself up, and stand erect, till the electricity of the conductor is exhausted. No repulsion.

4th Experiment.

To the conductor attach a metal rod terminating in a point. Present to this the inside of a glass, holding it in both hands; then place on a table some balls of pith of elder, cover them with the glass, and they will immediately begin to leap up against its sides. They will continue doing this for some time." 4th experiment.

In this experiment there is nothing extraordinary, and it is easily explained in the same manner as the others. The substance of glass has the property of setting itself in motion, and then attracting the electric fluid, both by friction and Explained on similar principles.

and communication with another substance saturated with this fluid: but I have observed, that to render it electrical one of its sides must be in immediate contact with a conducting substance communicating directly with the ground. Here the glass held externally between the two hands is nothing more than a Leyden phial: the pith balls are attracted by the power of affinity toward its charged sides; and the table, on which the glass rests, attracts them in its turn. Thus here again we have two attractions; whence the appearance of alternate attraction and repulsion, which continues as in the preceding experiments, as long as the interior sides of the glass are capable of furnishing a superabundance of fluid. Mr. Delamétherie has received from me a glass conductor twelve or fifteen inches long, which I sent him about six months ago, to give him a slight idea of my large glass conductors. Since that time I have found, that, on holding these little tubes by the middle, and keeping them in contact with the prime conductor for a few seconds while the machine revolves, the whole substance of the glass imbibes the electric fluid like a sponge: that we have time enough to carry the point, which is a little obtuse, and had been in contact with the machine, toward the knob of a Leyden phial held in the other hand: and that touching it about twenty times is sufficient to charge it as strongly as by so many sparks from the cap of a good electrophorus. I have not yet examined the nature of the fluid it gives, a subject that deserves a place in an essay on the Leyden phial, in which I intend to examine whether the phenomena be not more naturally explained by the hypothesis of one fluid, than of two. As the fluid in this case issues from a point, it does not exhibit sparks, but a kind of current, that is extremely brilliant.

Glass conductor.

Must not all these experiments lead us to suspect, that, if glass frequently appear to have no affinity for the electric fluid, it is because its bases, at the time of their uniting in the state of fusion, perfectly neutralise the igneous matter*? Consider

* Caloric, if you please, as Mr. Libes defines it in the article *Combined Caloric*, which is so interesting and short, that I cannot refrain from copying it. "It is that which intimately combines with the particles of bodies,

Consider, that by an electrification continued for a certain time it is not only reduced to the state of an excellent conductor; as every one, who will subject pointed tubes of glass to a powerful electrification for a time proportionate to the length and thickness of the glass, may convince himself; but that it is capable of supersaturating itself with this fluid, when one side of its surfaces is surrounded with a conducting substance, as metal, water, the hand, &c. Such is the Leyden phial, such the conductors of which I speak here when grasped in the hand, such the glass in this experiment of Mr. Libes, and such the glass balls of experiment 6. Every thing therefore tends to establish an analogy between the nonattraction of glass in the electric state and the neutralization of acids and alkalis, which is sometimes so perfect, that mixtures of two things that burn very powerfully when separate exhibit no sign of causticity. Experiments carefully made by Mr. Lugt, which I shall translate in my Essay on the Leyden phial, prove, that a communication with the ground is not necessary to charge it, but that it is sufficient to establish a sort of circulation between an electrical machine and an insulated phial.

Leyden phial may be charged while insulated.

5th Experiment.

5th experiment.

Electrical chimes.

"Hang three bells on a metallic rod furnished with a hook in the middle to suspend it from the conductor of an electrical machine. Two of these bells are to be fastened by a metallic chain to the end of the rod; the middle bell, and two small bells of metal hanging between it and the bells on each side, are to be fastened to the rod by a silk thread. From the inside of the middle bell depends a chain, which should reach to the floor, or be held in the hand during the experiment. Every thing being thus arranged, electrify the apparatus, and immediately the two little balls of metal will be attracted, each by its corresponding outer bell. Having struck this, they are immediately

bodies, and constitutes a part of their substance. In the act of combination it loses its physical properties, and is no longer perceptible by means of the thermometer." Is not this the phlogiston of Stahl under another name?

repelled

repelled toward the middle bell. These alternate motions will be repeated as long as the conductor of the machine is electrified."

Confirms the same theory.

These electric chimes form the strongest proof of my inferences. Examine the action of the insulated balls. They are attracted first toward the lateral bells saturated with this fluid: the middle bell, divested of superabundant electricity, and communicating with the ground, attracts toward it the electricity taken from the machine by the balls, and with it the balls themselves. This action may be compared with that of an electrical kite, which, attracting the electric fluid from a charged cloud, conveys it to the ground, if a metallic wire serve it as a conductor. Place an apparatus with two bells in the string of the electrical kite; one communicating with the conducting wire of the kite by means of a metal rod, the other insulated, but having a communication from its inside to the ground like the middle bell of the chimes: the moment the atmosphere has a charge of electricity, the kite will occasion these chimes to act like those of Mr. Libes, and gradually convey the electric fluid from the clouds by their means as well as by a conductor."

Electrical kite.

After having given these five experiments as proofs of two opposite actions, Mr. Libes finishes with these reflections on an experiment of Mr. Grey.

"These phenomena of electrical attraction and repulsion led Grey to the idea of imparting to a body by means of electricity an elliptical and at the same time rotatory motion. The following is a description of the apparatus he employed, and the true object of the experiment.

6th Experiment.

6th experiment.

"From the conductor of an electrical machine suspend a ring of stout brass wire about a foot in diameter. Beneath this ring place a circular plate of metal, supported by a stand, so that you can bring it near enough to the ring to prevent the glass balls, that will be mentioned, from slipping between the ring and the plate. On the plate place a ball of glass *blown very thin*, letting one point of its circumference be in contact with the ring. If the apparatus be electrified, we shall see the ball animated at once with a *rotatory*

a *rotatory* motion, and a circular motion round the plate; and if the experiment be made in the dark, the ball will appear luminous in every point in which it successively touches the ring."

General inferences.

This experiment singularly confirms the inferences I deduce from my experiments, that the nonconducting state of glass is owing only to the powerful affinity of composition of the component particles of good glass; and that when a powerful affinity, or the concurrence of several, presents itself to this substance, the latent igneous fluid is set in motion, and it becomes a conductor, or a body supersaturated with the electric fluid. When it presents itself in this state to a body, that has a strong affinity for the fluid which supersaturates it, it yields it up; but when the action is stronger, and burns, it is decomposed like all other substances; witness the long and continued action on the points of my conductors, and that of the solar rays on the glass of our windows. We see above, that the author requires the ball of glass to be very thin*: this is a necessary condition for producing the rotatory and revolving motion, for every thing made of glass in this state is moved by the slightest electric action; it kindles, as it were, like charcoal before the blowpipe; and being moved in one point, the neighbouring points tend by affinity to carry themselves in succession to the centre of activity. This is the natural way in which I explain these phenomena according to my manner of seeing them; perhaps by the *idola specus* of Bacon. But, si quid novisti rectius istis, candidus imperti—I seek only truth; this alone guides my pen, and in seeking it I write *commenta*, which perhaps time will efface. So many have been made of this kind in all countries, that the ancient Belgium cannot expect to be alone exempt from them.

* My conductors, when they are of thin glass, become excellent ones in less than a few hundred turns of the machine.

XVII.

XVII.

Analysis of the English Medicine called James's Powder. Communicated by Mr. C. L. Cadet, Apothecary.*

James's powder much used in Italy.

JAMES'S powder is greatly in vogue both in England and Italy. The English make a secret of it, and sell it for its weight in gold † as a sovereign remedy in asthenic diseases, and fevers attended with debility. Dr. Pearson, an English chemist, informs us, that he has analysed it; and that this powder is a triple salt composed of phosphate of lime and oxide of antimony. Those chemists, who have attempted to compose James's powder according to Dr. Pearson's recipe, have no doubt been astonished at their want of success. They must suppose, that the English physician, not to betray a secret from which his country profits, has not disclosed the whole truth ||; and in fact James's powder contains besides sulphate of potash and antimoniated potash.

The powder not to be prepared by Dr. Pearson's recipe.

Mr. Pully, of Naples, has analysed it afresh.

Mr. Pully, a Neapolitan chemist, who has rendered great services to the army of Italy as administrator of gunpowder and saltpetre, writes me word, that he has just analysed this powder, of which he procured from London a sufficient quantity to subject it to a strict examination. The following are the particulars of his experiments.

Warm water dissolved sulphate of potash,

Exp. 1. I took, says Mr. Pully, some James's powder, of which I weighed out 19 decigr. [29 grs.] After infusing a few minutes in a little warm distilled water, I filtered off the water, and obtained from it by evaporation a salt, that had all the characters of sulphate of potash. This salt, dissolved and treated with barytes, afforded a precipitate of sulphate of barytes.

* Annales de Chimie, vol. LV, 74. For Mr. Chenevix's paper on James's Powder, See Journal, vol. I, p. 22.

† The foreign venders appear to get a good profit on it, as the original price here is not above a fourth of this. T.

|| Dr. Pearson certainly concealed nothing in his analysis. T.

Exp. 2.

Exp. 2. As I perceived, before I decomposed the salt by means of barytes, that the solution contained an excess of free potash, I wished to ascertain, whether this potash did not hold in suspension a little oxide of antimony. In fact having decanted the fluid, to separate it from the sulphate of barytes, I poured into it some sulphuretted hidrogen, which immediately formed golden sulphur of antimony. The free potash, therefore, was combined with a portion of antimony at a minimum of oxidation. Dr. Pearson does not speak of this combination, or of the sulphate of potash. and some free potash holding oxide of antimony at a minimum.

Exp. 3. I took the James's powder, which had been washed with warm distilled water, and heated it with nitric acid of the strength of 20°. This acid dissolved the phosphate of lime, without attacking the oxide of antimony at a maximum. I separated this oxide from the solution, and poured into the liquor some ammonia, which precipitated the phosphate of lime. Nitric acid dissolved phosphate of lime.

Exp. 4. I decomposed the phosphate of lime with diluted sulphuric acid, and afterward recomposed it by means of lime water, in order to ascertain the proportions. Its quantity ascertained.

Exp. 5. I took the oxide of antimony at a maximum of oxidation, and dissolved it in muriatic acid. This solution, treated with sulphuretted hidrogen, produced a hidrosulphuret of antimony, containing more sulphur than kermes mineral, and less than the golden sulphur. Oxide of antimony treated with muriatic acid.

From these experiments, all the products of which I weighed, the 19 decigr. of James's powder, which I analysed, are composed of Component parts of James's powder.

	Parts.
Oxide of antimony at a maximum of oxidation	7
Phosphate of lime	4
Sulphate of potash	4·5
Free potash, holding oxide of antimony at a minimum	3·5
	19

To recompose this powder we must take Method of composing it.

	Parts.
Sulphuret of antimony	4
Calcined phosphate of lime	3
Nitrate of potash	8

These

These being powdered, mixed, and triturated together, they are put into a crucible, which is to be covered, and exposed to a strong heat. During this operation, the oxigen of the nitric acid, attacking the sulphur of the antimonial sulphuret, converts it into sulphuric acid, which unites with a portion of the potash, and forms sulphate of potash. The remainder of the free potash retains some antimony oxided to a minimum. The white powder, that is left in the crucible, is the same as that sold so dear by the English.

This affords the same products.

Mr. Pully informs me, that he has analysed his powder, to compare it with that of Dr. James; and that he has found it to contain the same principles, and in the same proportions.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Grasses.

AT the meeting of this Society on the 11th of March, Dr. Yule read an interesting memoir on the natural order gramineæ, with introductory observations on monocotyledonous plants, in which he contrasted these with the dicotyledonous class from the period of germination to the complete evolution of their stems. The Doctor is to continue the subject in another paper.

Plants with 1 and 2 cotyledons.

Scottish testacea.

Mr. Laskey laid before the Society a list of Scottish testacea, as far as they had fallen under his own observation, with remarks on the new and rare species. Of the genus chiton he enumerated 4 species; of lepas, 3; balanus 6; pholas 4; mya 9, including 3 new species; of ligula (a lately constituted genus) 7 species; solen 6; tellina 15, with a new species named by Colonel Montagu, *t. laskeyi;* cardium 10; mactra 6; donax 3; Venus 23, including 9 new species; chama 1; arca 6; pecten 6; ostrea 1; anomia 4; mytilus 11; pinna 1; nautilus 3; cypræa 1; bulla 13, including 2 new species; voluta 8, 4 of them new; buccinum

num 8; strombus 2; murex 23, comprehending the rare *carinatus*, and 3 new ones; trochus 4; turbo 32, 5 new; helix 17; nerita 7; haliotis 1; patella 11; dentalium 2; serpula 7; vermiculum 3. This is, upon the whole, the most ample catalogue of Scottish testacea hitherto framed, containing 126 species of multivalve and bivalve, and 142 of univalve shells; in all 268 species.

At the same meeting the secretary read a communication from George Montagu, Esq., of Knowel House, giving an account of a nondescript fish, 5 feet long, taken on the coast of Devonshire last summer. It must constitute a new genus, in the apodal order; and Mr. Montagu has bestowed on it the generic name of *ziphotheca*, and the specific one *tetradens*. The communication likewise contained accurate descriptions of four rare species of English fishes; and was accompanied with correct and elegant drawings of these, as well as of the ziphotheca.—At the same time Mr. Montagu presented the Society with copies of his Testacea Britannica, and Supplement, 3 vols. 4to, with coloured plates; and of his Ornithological Dictionary, 2 vols. 8vo. Nondescript fish.

TO CORRESPONDENTS.

THE learned author of the paper on comets in our last number, p. 206, is Mr. de Luc. His name was accidentally omitted from not being signed to the paper itself, but only to the letter in which it was enclosed.

METEOROLOGICAL JOURNAL

For MARCH, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

FEB. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
25	42	34	45	30	30·26	Fair	Fair
26	34	40	44	34	30·44	Ditto	Ditto
27	42	42	49	34	30·38	Ditto	Ditto
28	38	42	48	38	30·35	Ditto	Ditto
MAR.							
1	43	41	48	39	30·26	Cloudy *	Rain
2	42	38	46	32	30·36	Ditto	Fair
3	34	40	42	38	30·39	Fair	Ditto
4	42	43	46	38	30·16	Ditto	Rain
5	41	38	43	33	30·19	Cloudy	Ditto
6	34	36	40	34	30·26	Ditto	Fair
7	34	36	39	33	30·36	Ditto	Ditto
8	34	37	42	33	30·47	Ditto	Ditto
9	40	48	51	42	30·33	Ditto	Ditto
10	42	36	50	32	30·23	Ditto	Ditto
11	34	38	48	32	30·26	Ditto	Ditto
12	36	38	48	34	30·09	Fair	Ditto
13	36	40	44	37	30·21	Ditto	Ditto
14	40	40	44	38	30·24	Ditto	Ditto
15	38	42	46	34	30·44	Ditto	Cloudy
16	40	44	50	34	30·26	Cloudy	Fair
17	46	50	53	44	30·19	Ditto	Ditto
18	48	46	52	40	30·16	Ditto	Ditto
19	44	46	50	42	30·05	Ditto †	Ditto
20	44	45	50	41	29 97	Fair	Ditto
21	42	46	50	42	30·24	Ditto	Ditto
22	46	50	58	43	30·09	Ditto	Ditto
23	47	52	59	46	29·76	Ditto	Ditto
24	46	45	55	42	29·70	Rain	Ditto
25	44	43	50	38	29·34	Fair	Rain

* At 11 beautiful halo surrounding the Moon.
† Rain at 11 P. M.

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Mr. J. Bread's Timber Gauge

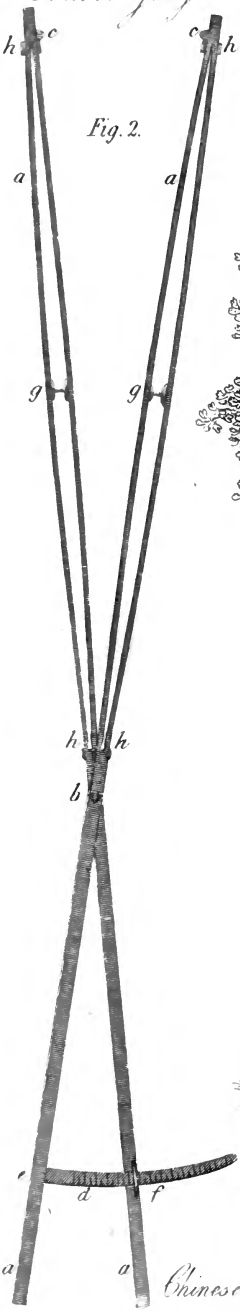


Fig. 2.

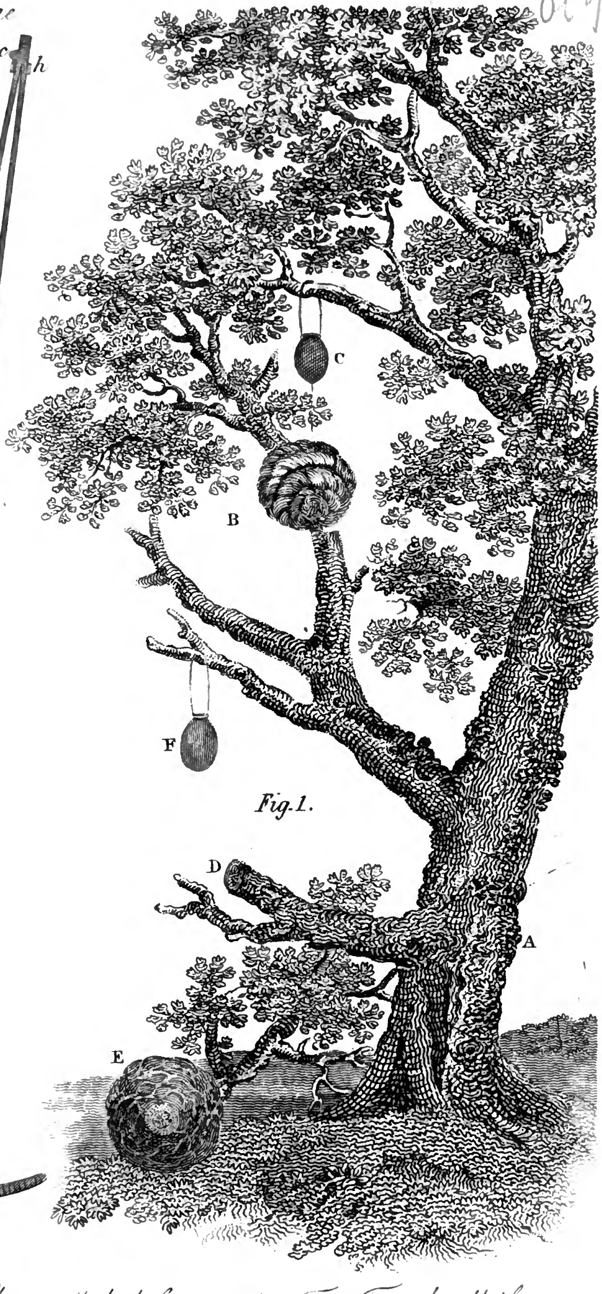


Fig. 1.

Chinese Method of propagating Fruit Trees by Abciss

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXII.

ARTICLE I.

An Account of the Chinese Method of propagating Fruit Trees by Abscission. By Dr. JAMES HOWISON *.

SIR,

THE Chinese, in place of raising fruit trees from seeds or from grafts, as is the custom in Europe, have adopted the following method of increasing them. Chinese method of propagating fruit trees.

They select a tree of that species which they wish to propagate, and fix upon such a branch as will least hurt or disfigure the tree by its removal.

Round this branch, and as near as they can conveniently to its junction with the trunk, they wind a rope, made of straw, besmeared with cow dung, until a ball is formed, five or six times the diameter of the branch. This is intended as a bed into which the young roots may shoot. Having performed this part of the operation, they immediately under the ball divide the bark down to the wood, for nearly two thirds of the circumference of the branch. A cocoa-nut shell or small pot is then hung over the ball, with a hole in its bottom, so small that water put therein

* Trans. of the Soc. of Arts, vol. xxv, p. 14.

will only fall in drops; by this the rope is constantly kept moist, a circumstance necessary to the easy admission of the young roots, and to the supply of nourishment to the branch from this new channel.

During three succeeding weeks, nothing farther is required, except supplying the vessels with water. At the expiration of that period one third of the remaining bark is cut, and the former incision is carried considerably deeper into the wood, as by this time it is expected that some roots have struck into the rope, and are giving their assistance in support of the branch.

After a similar period the same operation is repeated, and in about two months from the commencement of the process, the roots may generally be seen intersecting each other on the surface of the ball, which is a sign, that they are sufficiently advanced to admit of the separation of the branch from the tree. This is best done by sawing it off at the incision, care being taken that the rope, which by this time is nearly rotten, is not shaken off by the motion. The branch is then planted as a young tree.

In Europe a longer time required.

It appears probable, that, to succeed with this operation in Europe, a longer period would be necessary, vegetation being much slower in Europe than in India, the chief field of my experiments. I am, however, of opinion, from some trials which I have lately made on cherry trees, that an additional month would be adequate to make up for the deficiency of climate.

Advantages of this mode.

The advantages to be derived from this method are, that a further growth of three or four years is sufficient, when the branches are of any considerable size, to bring them to their full bearing state; whereas, even in India, eight or ten years are necessary with most kinds of fruit trees, if raised from the seed.

When at Prince of Wales's Island, I had an opportunity of seeing this proved by experiment. Some orange trees had been raised by a gentleman, from seeds sown in 1786, which had not borne fruit in 1795, while branches taken off by the Chinese mode in 1791, had produced two plentiful crops.

Whether

Whether forest trees might be propagated in Europe in the same manner, I have not had experience sufficient to form a judgment: if it should be found practicable, the advantages from it would be great, as the infancy of trees would, by this means, be done away, a period which, from the slowness of their growth, and the accidents to which they are liable, is the most discouraging to planters. Applicable to timber trees;

The adoption of this method will, at all events, be of great use in multiplying such plants as are natives of warmer climates, the seeds of which do not arrive here at sufficient maturity, to render them prolific. and natives of warm climates.

I have frequently remarked, that such branches of fruit trees, as were under the operation of abscission during the time of bearing, were more laden with fruit than any other part of the tree. It appeared to me probable, that this arose from a plethora, or fulness, occasioned by the communication between the trunk and branches through the descending vessels being cut off by the division of the bark, while that by the ligneous circles or ascending vessels, being deeper seated, remains*. The same reasoning accounts for fruit trees producing a greater crop than usual, on being stripped of their leaves, most of the ascending juices being thrown off by them in perspiration, or expended in their nourishment, for we find that bleeding trees cease to give out their juices after they have put forth their leaves †. These branches bear well. Stripping fruit trees of their leaves.

I have observed, that the roots from a branch under the operation of abscission were uniformly much longer in shooting into the rope when the tree was in leaf, than the contrary; hence, the spring season appears most proper for performing this operation.

* The circumstances attending the Chinese method of propagating fruit trees appear a strong confirmation of Mr. Bonnet's opinion, that plants, as well as animals, have a regular circulation of their fluids.

† Marsden, in his History of Sumatra, page 119, says, "The natives, when they would force a tree that is backward to produce fruit, strip it of its leaves, by which means the nutritive juices are reserved for that important use, and the blossoms soon show themselves in abundance."

 It

Fruit without seed obtained by dividing the pith of trees.

It will seem singular, that the Chinese entertain the same opinion that Linnæus did, respecting the pith of trees being essential to the formation of the seed. By cutting into the trunk of the guava tree before it has produced, and making a division in the pith, they have obtained fruit without seed.

I am, Sir,
Your obedient Servant,
JAMES HOWISON.

Reference to the Engraving, Plate IX, *Fig.* 1, *of the Chinese Method of propagating Fruit Trees by Abscission.*

Explanation of the plate.

A. The tree on which the operation is performed.

B. The straw rope wound in a ball round a branch of the tree.

C. The cocoa-nut shell, or vessel, containing the water, which gradually drops thence on the ball below it.

D. Another branch of the same tree, from which the part E, rooted in the straw rope or ball, and now ready for planting out, has been separated.

F. The vessel suspended from a branch above, and from which the ball has been supplied with water.

II.

Description of a Gauge or Measure for standing Timber, invented by Mr. James Broad, *of Downing Street* *.

SIR,

Standing timber liable to be estimated very erroneously,

THE Instrument I send herewith is for finding the girth of standing timber, and will, I flatter myself, be found exceedingly useful to all gentlemen, and others having timber to dispose of, and likewise to such purchasers as wish to pay for the true quantity. At present a gentleman having timber to dispose of is liable to be imposed on to a very large amount; for though some surveyors may be found whose eye is pretty accurate, yet this is far from being

* Trans. of Soc. of Arts, vol. xxv, p. 18.

generally

generally the case. When an estate is sold on which the timber is to be valued, I believe, there is no other way in general use of finding the girth of a tree (which, being squared and multiplied by its length, gives the contents) than by actually getting up to the middle, where the girth is usually taken, with a ladder or otherwise: a method which is very troublesome and expensive where the quantity is large. The seller has, therefore, no way, but at an enormous expense, of finding the real contents of what he has to offer, and as the buyer, if a dealer, from his knowledge is able to form a more accurate judgment, it often happens, that the seller sustains much loss. *I have known it exceed 50 per cent.* Having some time ago a large quantity to survey, I thought it possible to invent an instrument, which would obviate this inconvenience, and which might be sold at a low price, be correct in its work, quick in execution, and such as any capacity might use. I likewise thought it might be so contrived, as to make such an allowance for bark, as should be agreed on. The instrument I send you possesses all these qualifications, and is susceptible of several improvements, of which I was not aware when I made it, which I will point out at the end of my letter.

unless measured at great expense.

It is well known, that the diameter and circumference of circles are in a certain proportion to each other, and that double the diameter gives double the circumference. The allowance for bark is usually one inch in thirteen, that is, if the greater circumference of a tree with the bark on is found to be thirteen inches, it is supposed it would be only 12 inches if the bark was taken off.

Allowance for bark.

The instrument is composed of two straight pieces of well seasoned deal, about thirteen feet long, joined together by a pin going through them, on which they are movable; but neither the length nor thickness is of any particular consequence, as, by following the directions hereafter given, they may be made of any size. A little way from the larger end is a brass limb, I call the index, on which are engraven figures denoting the quarter-girth in feet and inches. To use this instrument, it is only necessary to take hold of the large end, and apply the other to that part of the tree where you wish to know the girth, opening it so wide as just to touch

Measuring instrument described.

at

at the same time both sides of it, without straining it, keeping the graduated side of the index uppermost, on which the girth will be shown, after allowing for the bark, by the inner edge of the brass on the right hand leg. An operation so easy and simple, that a person of the meanest capacity might measure a great number of trees in a day.

Instrument for measuring the height.

For taking the height of a tree, I would recommend deal rods of seven feet long, made so as to fit into ferrils at the end of each other, tapering all the way in the same manner as a fishing rod. A set of five of them, with feet marked on them, would enable a man quickly to measure a tree of more than forty feet high, as he would be able to reach himself about seven feet.

Improvements, that might be made in it.

The improvements it is capable of are, making a joint in the arch or scale, to enable it to shut up (when the legs are closed) towards the centre, which would make it easier to carry. Secondly, as it sometimes happens, that standing timber is sold without any allowance for bark, and at other times with a less allowance than one inch in thirteen, two other scales on the index might be added in such cases, one without any allowance, and the other to allow as might be agreed on. I would have added these, but thought the Society would rather see it in the state in which it has been tried on a large survey, as any artist can with great ease add whatever scale he pleases. The present scale allows one inch in thirteen for bark, and is calculated on the following data. The diameter of a circle the quarter circumference of which is 26 inches, is $33\frac{90}{100}$ inches. The diameter of a circle, the quarter girth of which is $6\frac{1}{2}$ inches, is $8\frac{27}{100}$ inches. To graduate the scale, the instrument is opened so as to take in at the small end between the touching points $8\frac{27}{100}$ inches, and a mark is made on the arch to denote 6 inches quarter girth: it is then opened so as to take in $33\frac{96}{100}$ inches, and another mark is then made on the arch, to denote two feet quarter girth; (these marks are made close to the inner edge of the brass on the right hand limb): the space between them is then divided into eighteen parts, which represent inches, and are again divided into halves, for half inches; if any notice is to be taken of quarter inches, the eye will easily make a farther decision.

I beg

I beg leave to add, that it is not my intention to make any for sale.

I am, Sir,
Your obedient Servant,
JAMES BROAD.

Reference to the Engraving of Mr. James Broad's Machine for measuring standing Timber. Plate IX, Fig. 2.

Fig. 2. *aaaa* Two long pieces of well-seasoned wood, joined near the middle by a pin *b* going through them, forming an axis on which they move. *cc* Two pieces of brass screwed near their upper ends, on the sides opposite to each other, and projecting over to form the measuring points. *d* The index fastened to one of the pieces of wood at *e*, and moving freely under a small bar at *f*. *gg* Screws with nuts, placed in the middle of the long slits of the two arms, to wedge them open, whereby the vibration is destroyed, and the arms, though light, are rendered stiff. *hhhh* Screws and nuts to prevent the arms from splitting. Explanation of the plate.

A certificate from Mr. J. Wilkins, carpenter, of Sandy Lane, dated May 4, 1805, stated, that he had used the instrument invented by Mr. J. Broad, for measuring timber standing, and that he believes it to be a correct and valuable one.

III.

Report of a Committee appointed by the Bath and West of England Society, to investigate the Claim of the Right Hon. Lord Somerville to a Premium "for the greatest Number and most profitable Sort of Sheep."*

YOUR Committee report, that the claim is founded upon facts, as under:

About the year 1800, Lord Somerville's stock (as stated by him in his Memorial to the Society) consisted of forty-five ewes of the long-woolled sort. Finding these annually Lord Somerville's stock of sheep,

* Bath Society's Papers, vol. X, p. 71.

degenerating,

changed for Ryeland.

degenerating, and also becoming annually less profitable, he changed them at the above-mentioned period for one hundred and fifty Ryeland ewes.

Improved.

In the first year, though the winter was severe, the ewes supported themselves tolerably well, and the lambs were in very good order at weaning time. In their future growth, as wethers and store ewes, they far exceeded in weight their parent stock. One lot of the wethers sold as high as 3l. each, and were fed upon grass and hay only.

Mixed with Merino.

In the following year, Lord Somerville brought from Spain some rams and ewes of the Merino breed. These rams, in each subsequent year, have been, and now continue to be, put to ewes of the South-Down and Ryeland breed; from each of which crosses a valuable species of sheep has been obtained, both in fleece and carcase; the relative value of which has been detailed by his lordship in his memorial of 1802, the substance of which is, that

Profit of South-Downs;

South-Down store-ewes at 3*lb.* per fleece, and at 1*s.* 10*d.* per *lb.*, will pay 5*s.* 6*d.* per fleece; which, at 6½ per acre in good upland pasture for seven months, and five months in turnips at 14 or 15 per acre, will pay 38*s.* or 40*s.* per acre.

of Ryelands;

Ryeland store-ewes 2¼*lb.* per fleece, at 2*s.* 2*d.* per *lb.* untrinded, nine sheep per acre, and turnips as above, will pay 2*l.* 3*s.* 10½*d.* per acre.

of South-Down and Merino;

South-Down and Merino ewes of the half-breed, at 4*lb.* per fleece clean washed, and 3*s.* per *lb.*, will amount to 12*s.* per fleece; which, at 7½ per acre for seven months, amount to 4*l.* 10*s.* per acre for the pasture land, with turnips as above for winter keep.

of Ryeland and Merino;

Ryeland and Merino ewes of the half blood at 10 per acre for seven months, and turnips as above, at 3¼*lb.* per fleece, and 3*s.* 2*d.* per *lb.*, amount to 6*l.* 10*s.* 5*d.* per acre.

Pure Merino.

The pure Merino fleeces never sold at less than one guinea each; the average weight of which has been more than 6 *lb.* each in the yolk; and on the above allowance of pasture for seven months, and turnips as above in aid of that pasture, the return will amount to ten guineas per acre.

Size of the farm.

The size of the farm in Lord Somerville's occupation is four hundred and sixty acres, eighty-five of which are a dairy

dairy unfit for sheep, except for a few couples in the spring. These sheep have been depastured as under:

56 Acres one and two years old clovers, indifferent keep, some worn-out ley. Feed for sheep.

85 Acres marsh and capital pasture.

35 ditto upland summer pasture.

5 ditto just taken in hand, foul.

7 Keep upon thirty acres of water-meadow for six weeks in the spring, equal to one fourth the number of acres, or seven.

Total - 188 acres; beside the run of thirty-three acres of turnips. But it is to be observed, that in the same ground sixteen plough oxen occasionally, but twelve constantly, were depastured; four horses occasionally, four cows constantly, till the after-grass: to this is to be added the run of yearling calves, and a large stock of pigs; and that the green crops of the spring and summer, 1803, were unusually deficient in these districts. The sheep stock amounted to

Other stock on the same ground.

Sheep stock.

302 lambs,
783 store sheep,

Total - 1085;

The produce of them as follows:

Produce.

Wool, 12 packs, 1 score	*£446	0	0
216 store-sheep sold	409	3	0
132 fat sheep ditto and used	238	16	2
Letting of rams	524	10	0
	£1618	9	2

N. B. No sheep have ever been kept fattening older than four teeth. The fat sheep were all sold after being sheared, and the price of mutton, from the deficiency of keep, was not more than $4\frac{1}{2}$*d.* per *lb.* at one part of the season.

No sheep older than four teeth fatted.

J. F. LUTTRELL.
JOHN BRICKDALE.
W. JONES

Fitzhead, June 2, 1804.

* Five hundred fleeces of the above, at 15*s.* 8*d.* per fleece, amounted to 392*l.*

Omitted

Omitted to take into account keep* of sheep in Sir J. Trevelyan's park, 18*l.* and about six tons of hay, at two guineas per ton, 12*l.* 12*s.* Per contra creditor, for turnips given to two bulls, 4*l.* 12*s.* leaving a balance of 26*l.* to be deducted from the above

£1618	9	2
26	0	0
£1592	9	2

IV.

On the Advantages of the Use of Oxen and Neat Cattle in Husbandry. By LORD SOMERVILLE †.

SIR,

London, Dec. 8, 1804.

YOU will have the goodness to express to the Bath Society my regret, that I cannot attend its anniversary meeting, as was my intention.

Change of sheep stock.

On the subject of my claim to the premium for a change of sheep stock, &c.‡, I have only to observe, that it was made at a period the most unfavourable to stock, when distress for keep of all sorts was greater than I have ever known; and that whether the premium be adjudged or not to me, I shall ever consider the favourable reception it met from the committee to which it was referred, the able essays which it produced, and the recommendation which this committee, numerously attended, has unanimously given to this general meeting that it should be awarded, together with the proofs since produced, as decisive on this most important question.

Return of labour with oxen.

The return of my year's labour with oxen was made out for the year 1803, because the year 1804 not being expired, to have made it without the amendment, since resolved on, would have been a palpable errour. I now comply with the repeated and earnest wishes of the society, in presenting this statement for its inspection.

* Occasioned by the total failure of grass in the marsh, from the severe draught in September.

† Bath Society's papers, vol. x. p. 56. ‡ See the preceding article.

Unprepared

Unprepared as I was, it would have been impossible to have done it with that degree of accuracy, which I shall adhere to in any statement of serious import to the public; but the rules which govern the proceedings of the Bath Society allowing me time to examine my own books, and to obtain replies to certain needful questions, I have great pleasure in stating, that the following report is now presented to the society, in substance, I trust, correct. In that part of the statement, which reduces the hauling and carting of manure to be equal to a given number of acres ploughed, I have profited by the kind assistance of Mr. Paul, and Mr. Gordon Grey. Correct.

No land ploughed with horses, save part of one acre, as a trial.

Work done by twelve oxen in one year.

	No of acres.
Forty-four acres of ley ground broke up -	44
Fifty ditto of spring corn, two earths, scarified and dragged, equal to 1½ earth more - - - - - - -	170
Sixty ditto of turnips, at three earths, cultivated, or scarified and dragged, equal to 1½ earth - - - - - - - -	271
Twenty-two ditto pease, at one earth, and broad-cast dragged, part of it drilled -	26
Fifty-seven acres of wheat, 32 acres on one earth, once dragged, and twenty-five of heavier land, twice ploughed and twice dragged - - - - - - - -	123
Twenty-nine ditto of ley ground, broke up to December 1st - - - -	29
Three ditto of beans, seven ditto winter vetches, two ditto potatoes, cultivating, dragging, &c. - - - - - -	17
Lime carried and "butted".* over the land, 160 butts, equal to two acres work, being a large proportion of 1920 hhds. or 9600 bushels of lime - - -	120 Hauling 125 Butting *
Carried forwards	925

* Butting. The lime being mixed with the forehead or headlands, round the edges of the field, is thence carried in implements called butts, bodily over the land—a laborious but good management.

Brought forward	925
Twenty-five acres of the turnip land dunged, equal to $30\frac{1}{2}$ - - - - - -	$30\frac{1}{2}$
Total	$955\frac{1}{2}$

Hay-harvest, corn-harvest, corn to market, hauling of timber, &c. not easily calculated, but still to be allowed for, probably amounting to

Allowing for the three-year-old steers ninety acres, and eight half-days work of two two-year-old bulls, worked occasionally with the steers for the purpose of keeping them quiet; allowing also as above the hay-harvest &c. not brought to account; it will appear, that the labour of the twelve oxen throughout the year will amount to, if not exceed one thousand acres.

Average of labour in a day.

The average amount of our labour is two acres of ley ground per day, and fallowing and stirring more than two acres; but the second cross ploughing, or earth, somewhat less. Our teams consist of four oxen, a man, and a boy, to each double-furrow plough, and to each four-wheel waggon. Our ploughing in general very deep, and our fields small, not exceeding $4\frac{1}{2}$ acres each on the average. These two circumstances are to be duly considered as adding materially to the labour. The working stock consists of sixteen steers and oxen, two bulls, and three light horses, *viz.* six five-year-old oxen, six four-year-old steers, and four three-year-old ditto. They are fresh growing stock, and are regularly turned out to graze after the barley-sowing, at six years old. The whole object aimed at is to carry on our course of crops on the most speedy and vigorous system, but without injury to the growth of the stock.

Working stock.

This labour not injurious.

It is evident, that my labour, severe as it long has been, cannot be found to injure the health or growth of the stock: the exhibition of my oxen annually, within ten months grazing from the time they are turned out of work, will fully exemplify this important fact. This was in great part my object in establishing the *Barbican* cattle-shew; and if I may be allowed to say so, the effect already produced, more especially in countries where oxen were held in

Barbican cattle shew.

in disrepute as animals of labour, has exceeded my most sanguine expectations. That our crops are worked in so expeditiously as to amaze those who contend for horse-labour only, cannot be denied; in proof of which, fifty-seven acres of wheat were this autumn ploughed, sown, and manured in a complete manner, according to the usage of the country, within three weeks, although the weather was unfavourable, and the land worked close and heavy. The last nine acres were ploughed, sown, dragged, and harrowed, in one day. In obedience to the wishes of the society I present them with this statement; but I wave all claim to a premium. If, however, in the ordinary course of business, any man in this kingdom shall be found to have done more at a less cost, I shall consider myself as having trespassed unworthily on the notice of this society. It is fitting to add, that in twenty years labour I have not lost one ox or steer, or ever broke a yoke or pair, by sickness, death, or accident. And I may farther add, that, so far from incurring any loss of value from working cattle after their full growth, as is supposed to be the case with horses, amounting to 25 per cent or more; my own experience, and the concurring opinion of the committee sent to examine our stock in the month of June last, warrant me in declaring, that working-cattle, from three to six years of age, do actually gain at the rate of 20 per cent yearly; the loss in my own case, in twenty years, being nothing!

Superiority of oxen.

Not one beast lost in twenty years.

Average gain of working cattle.

The premium now in question, having distinctly waved my claim to it, will probably be awarded to Mr. Billingsley; and it gives me sincere pleasure, that it should be bestowed on him. He has been a most strenuous and successful advocate for the labour of oxen, and is well aware of its extreme importance. Mr. Billingsley has accomplished a measure hitherto untried, namely, to set out his ploughing by the acre, and to apply one team of oxen, full grown, with two to assist, in all eight oxen, to constant plough-labour, every day in the year that it was possible for them to work. I consider the attempt of such consequence to the landed interest, so momentous an illustration of the powers of these superior animals in labour, that I beg

Mr. Billingsley

sets out his ploughing by the acre.

 leave

leave here to offer him my sincere thanks; and I have the honour to be,

With all respect to the Society, &c.

SOMERVILLE.

V.

Practical Statement on the foregoing Subject, with Claim of Premium. By JOHN BILLINGSLEY, ESQ.*

Work done by a team of six oxen in eleven months.

THE claimant states, that his servant Esau Green has on a farm of eight hundred acres, the soil of which is of a middling texture, ploughed and harrowed, with a team of six oxen and a double-furrow plough, the following acres of land, *statute-measure*, between the 1st day of January and the 1st of December, 1804, *viz.*

Acres.	
56	of oat stubble.
62	of turnips for oats.
68	of ley.
68	of ley cross-ploughed for a fallow.
100	of fallow, for the purpose of being cleaned after a a slovenly tenant.
19	of vetches.
12	of vetches folded off, and sown with wheat.
385	acres ploughed in eleven months.
56	of oat stubble.
62	of turnip sown to oats.
14	of ley.
80	of fallow.
48	of cross-ploughed ley.
19	of vetches.
12	of wheat.
291	acres harrowed in the same eleven months.

* Bath Society's papers, vol. x. p. 61.

The

The claimant farther states, that no possible errour can have crept into this statement. No other team being employed in the tillage of this farm, and the ploughman being paid by *the acre*. Nor can any doubt arise respecting the size of the acres, as it is well known, that lands newly enclosed are set out *statute-measure*. Statement accurate.

The oxen employed were home-bred, of the long-horned race, and were purchased last year of one of the claimant's tenants, at the price of 14*l*. each. Four of them were six years old, and four four years old. Oxen employed.

Though eight oxen were kept, six only were worked at a time. The other two were changed as occasion required, at the will of the ploughman. These oxen are in no respect injured by their labour, and are now in good working order. Not injured by their work.

The ploughman and driver were paid 1*s*. 4*d*. per acre for ploughing, and 6*d*. per acre for harrowing; and in this was included all necessary attendance on cattle at all times and seasons. Price of labour.

The depth of ploughing from 3 to 5 inches.
The breadth of ditto from 7 to 10 inches.

As the harrowing was all performed with six oxen, drawing very heavy and long-tined harrows, (provincially called drags) and in many fields two bouts in a place, it will not be unfair to estimate *two* harrowings as equal to *one* ploughing; and in proof of this comparison it may be observed, that the double plough will turn two acres and a half in eight hours, which are half as much as six oxen can harrow in the same time. Harrowing.

Presuming that no solid objection can be brought to the foregoing statement, it may be satisfactory to the society to see the debtor and creditor sides of the account, methodically arranged, so as to ascertain the cost both of the ploughing and harrowing per acre, *statute-measure*. Statement of expence.

Dr.

Eight Working Oxen, from Jan. 1st to Dec. 1st, 1804.

	£.	s.	d.
To 24 lb. of hay per week, consumed between Jan. 1 and May 12, when they were turned to grass, at 2*l*. 10*s*. - - - - - -	47	10	0

To

Brought forward	47	10	0
To 24 week's keep at grass, at 3*s*. each ox -	28	16	0
To 6 tons of hay, between Oct. 1 and Dec. 1, when at grass - - - - - -	15	0	0
To repair of yokes and bows - - -	0	18	0
To wear of plough and dressing shares, mending chains, &c. - - - - - -	4	13	6
To cash paid Esau Green for ploughing 385 acres in eleven months, at 1*s*. 4*d*. - - -	25	13	4
To cash for harrowing 291 acres, at 6*d*. - -	7	5	6
	£129	16	4

N. B. This price per acre includes driver and attendance.

Cr.

By 385 acres ploughed, at 4*s*. 10¼*d*. - -	93	8	10
By 291 acres harrowed, at 2*s*. 6*d*. - -	36	7	6
	£129	16	4

Saving.

If this had been let by hire, it would not have been taken by any neighbouring farmer at less than 8*s*. per acre ploughing, and 4*s*. per acre harrowing.

Proportion of work.

If the harrowing of *two* acres be admitted as equal to ploughing *one*, it follows, that the work done by these oxen, (caparisoned in the old-fashioned way with yokes and bows) is equal to the ploughing of 530 acres in eleven months, or 578 acres in a year.

Earnings of the man and boy.

It may naturally be supposed, that on so elevated and exposed a hill as that of Mendip many interruptions to tillage work must occur in the course of a year, notwithstanding which the man's earnings (driver included) amount to 14*s*. per week nearly; to which must be added sundry work in the time of harvest, (after his day's work at ploughing) and other occasional labour, amounting to the average of 2*s*. per week, constituting altogether the receipt of 13*s*. per week for himself, and 3*s*. per week for his boy.

In conducting this experiment the claimant has sedulously guarded his mind against all bias and partiality, being desirous of getting at the truth by the most accurate investigation; and he has no doubt, that the statement of work performed

performed under the direction of a noble lord (the rival candidate for this premium) will lead to the same conclusions, and rescue this most useful animal from that degraded state of inferiority, in which he has unjustly been classed.

At Midsummer last the claimant had not the most distant idea of starting as a candidate; and since that time he has in no way whatever stimulated the ploughman to *extraordinary* exertion.

Advantage of ploughing by the piece.

In letting his tillage labour by contract he has not departed from the uniform practice of twenty-five years. A practice originating from an idea, that the best method of making servants laborious and honest is to make it their interest to be so.

Encouraging however as these facts must be to the use of oxen in this department of husbandry, he does not venture to say, that they will answer in all countries, or on all soils.

On some soils horses may be nearly as cheap as oxen.

On all light sandy soils, such as Norfolk, Suffolk, &c. single ploughs of different constructions, drawn by two horses without a driver, may in cheapness of execution nearly approach the double-furrow drawn by four oxen. But on all level soils unincumbered with stones, and where good pasture may be found for *summer*, and good hay for *winter* keeping, oxen with the double-plough may in his opinion be considered as justly entitled to a preference.

JOHN BILLINGSLEY.

Bath, Dec. 6th, 1804.

VI.

On the Conversion of French Weights into English, in a Letter from MR. JOHN FAREY.

To Mr. NICHOLSON.

SIR,

Tables of reference should be correct.

THE importance of having tables of constant reference as free as possible from errours will I hope plead my excuse for troubling you on this occasion, and obtain insertion for this in your Journal, in order to point out two very errone-

 ous

ous columns in the table, which you gave in your 4to Journal, vol. I, p. 332, for reducing *grammes* of the new French system of weights into English grains, and vol. II, p. 284, for reducing English grains into grammes.

The French gramme erroneously given.

The errours here alluded to seem to have originated at p. 199 of your first volume, where one gramme is stated to be 22·966 English grains, instead of 15·457, which results from multiplying 18·841, the number of French grains, by ·8204.

Corrected in the British Cyclopædia.

Since the above determination of the *gramme*, the completion of the meridional measurements has introduced a small correction, which occasions, I suppose, the gramme to be stated at 15·444, in the article GRAMME in your new *British Cyclopædia*, which is the proper reciprocal of ·06475, stated therein, under the article WEIGHT, as equivalent to 1 grain troy; but in your 4to Journal vol. II, p. 284, the whole column is calculated from ·04354 instead of the above.

Corrections of other weights.

Permit me here farther to remark on the article WEIGHT, in the last part of the *British Cyclopædia* just published, that 1 pound troy is 288 scruples; and not 283; that 1 scruple is 1·295 grammes and not 1·395; 1 ounce avoirdupois is 28·328 grammes, and not 28·32; 1 dram is 27·34375 grains, and not 37·975: and 1 dram is also 1·7705 grammes and not 1·81. The English troy pound contains or equals 7021 Paris grains nearly, and not 702; the English avoirdupoise pound is 8532·5 French grains, and not 8538, according to the table on the opposite page thereto: the first column of which table is inconsistent with (as not a true reciprocal to) the second column; for 1 French grain is 0·8204 English grains, as is rightly stated on the preceding page, and not ·8203; 10000 French grains are 8204 English grains, and not 8233, as there stated.

As unfortunately the conclusion of the British Cyclopædia prevents the *errata* being there given, I have set them down here, and am,

SIR,

Your obedient humble Servant,

JOHN FAREY.

12, *Upper Crown Street, Westminster.*
12th January 1809.

VII.

VII.

Account of an Improvement in Tram Plates for Carriages on Rail Roads. By Mr. CHARLES LE CAAN, *of Llanelly in Wales**.

SIR,

I HAVE forwarded to the Society of Arts &c. a specimen of my new method of laying rails, or tram-plates, on such a plan as has met the entire approbation of those who have seen it, and are acquainted with the principle on which such roads should be formed. Rail roads are daily increasing, from the great advantage they afford to manufactories connected with mines and minerals, and particularly to collieries. They also promote Agriculture, by occasioning lime to be procured from places almost inaccessible by any other means, or whence it could be otherwise brought on moderate terms.

Rail roads much increasing from their advantages.

I flatter myself, that every improvement on this system will be of national importance. The honour I received last year from the Society of Arts &c. has stimulated me to submit the present subject to their consideration.

I have also sent a drawing of my method of laying the tram-plates, with an estimate of the saving that will arise to the public, by adopting the said method, with necessary remarks on the principle on which it is founded. The leading rail or tram-plate has neither tenon nor mortise over the plug. The stop-plate terminates the specimen, which stop-plate should go in with some degree of tightness when laid for actual use, but in the present case that force is not necessary, as the wooden blocks, by a carriage of upwards of 200 miles, may in some small degree be misplaced. I hope any impediment of that nature will be rectified or allowed for. I wish it to be understood, that a stop-rail is intended to be placed at every 30 yards, at which distance any repairs may be made within ten minutes, which by the present mode frequently occupies more than twice that time, exclusive of disturbing in some measure the line of road

Improved rails or tram plates.

* Transactions of the Society of Arts, vol. xxv, p. 87. Twenty guineas were voted to Mr. le Caan for this improvement.

road. By my method, the plates have a certain degree of play, which is absolutely necessary to avoid that breakage, which too frequently takes place when they are fixed with nails and plugs.

The plates which I send have been fixed in stone blocks, and are nearly as rough as when taken from the sand. If I am favoured with any mark of the society's approbation, I shall hold myself bound to transmit such farther communications on this subject as may be required by them, or any person desirous of adopting my plan.

I am, Sir,

Your most obedient Servant,

CHARLES LE CAAN.

Llanelly, Carmarthenshire,
May 12, 1806.

SIR,

Advantages of them.

I HAVE considered the improvement made by you in the specimen exhibited of a new design of a tram-plate, and am of opinion, that much advantage may be derived to tram roads by the adoption of your plan, in preventing the temptation of stealing the wrought iron nails, with which the plates are usually fastened, and by facilitating the operation of laying down new tram roads, and repairing of old ones.

I am, Sir,

Your most humble Servant,

J. VANCOUVER.

Llangennech Park, April 2, 1806.
To Mr. le Caan.

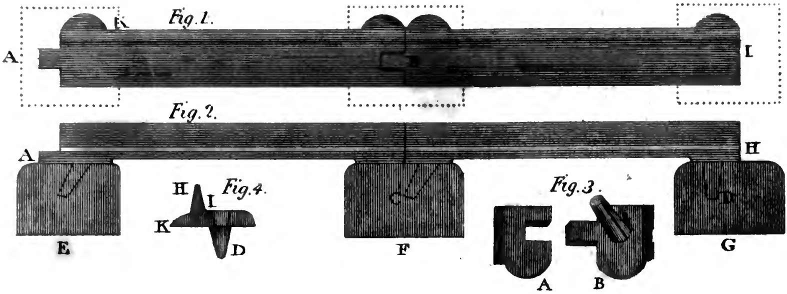
SIR,

Advantages.

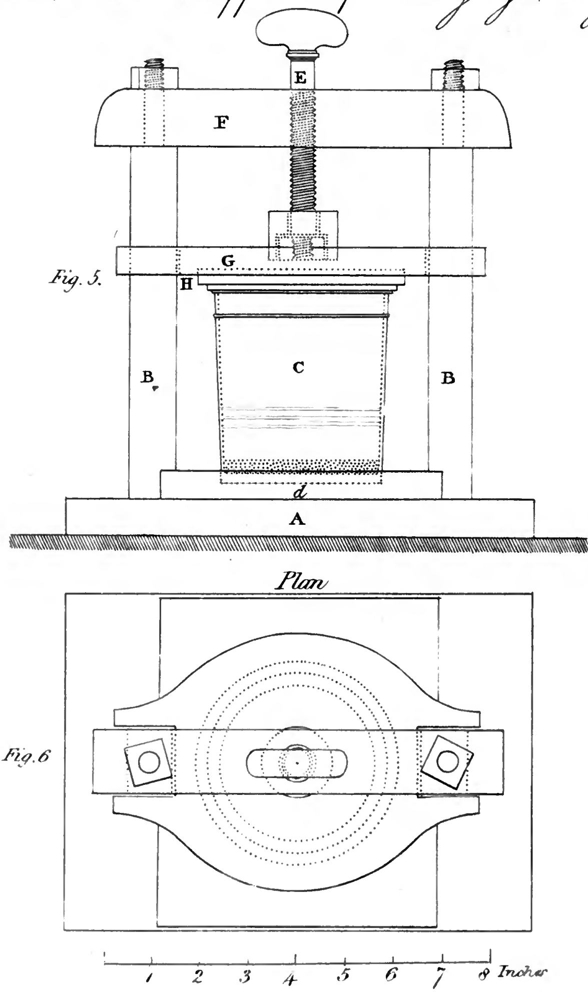
SEVERAL tram-plates on your new method of fixing without either nail or plug have been cast under my immediate inspection, at Stradey furnace. The same may be made with as much ease as any others now in use, and I conceive they will obviate the many impediments that arise

2 from

Mr. Charles Le Caan's Tram Plates.



Guyton's Permanent Apparatus for destroying Contagion



from the irregularity of driving the nails. I have no doubt from my observation of yours, but that they will answer extremely well, and prove less expensive.

I am, Sir,
Your obedient Servant,
J. LEWIS.

Stradey Furnace, April 18, 1806.
To Mr. le Caan.

Further testimonies.

Certificates were also received from Mr. R. Jones of Swansea, agent to General Ward's colliery; from Mr. James Barnes, who formed the Myther and Carmarthenshire rail road; and Mr. Edward Martin, of Morristen, an eminent colliery, surveyor and planner of rail roads, all testifying the advantage of Mr. le Caan's invention.

Reference to the Engraving of Mr. Charles le Caan's *improved Tram-Plates for Rail-roads. Pl* X, *Fig.* 1, 2, 3, 4.

Description of the tram-plates and method of making rail roads.

The tram-plates, fig. 1 and 2, are fastened by means of a tenon and mortise A B, each having a correspondent bevel, just sufficient to keep the end from rising up, so that the head of one plate confines the end of the other; by this means, the workmen are obliged to form their road in right lines, and maintain perfect levels, as the mortise and tenon confines them to the required exactness necessary to make a perfect road: curves or any given segment may be formed with the same nicety, by having two bevel rails or plates made for such purposes.

Fig. 2, A side view or longitudinal section of the two plates placed on their stone blocks or sleepers C D, shows two plugs in dotted lines, one bevel, the other perpendicular, cast in the stop-rail or plate, which is so called as it prevents the others from moving, and when taken up releases all those between the stop-plates; 25 yards of rail road made with these plates, may be taken up and replaced within ten minutes. The plugs in dotted lines are shown in their proper positions within the sleepers E F G.

The

Description of the tram-plates and method of making rail roads.

The usual length of a tram-plate is three feet, the flanch or outside edge H, about one inch and half high, the sole or bed I, from three inches and a half to four inches broad, and three fourths of an inch thick; but these dimensions may be varied according to circumstances. The most approved weight has been 14 pounds to the foot, or 42 pounds to the plate, the ends from which the plugs project, and to which the tenons and mortises fasten, should be one fourth of an inch thicker than the other part of the plate.

Fig 3. A B show the under part of the tenon and mortise, and the form of one of the sloping or bevel plugs.

The diameter of the plug near the shoulder is one inch and three quarters, reducing to one inch, its length two inches and a half, forming an angle of eight degrees, the plate from which it projects is counter sunk, so that the shoulder of the plug may not receive any sharp pressure or prevent the plate from having a perfect bearing. There is a small groove in the whole length of the exterior of each plug, to admit a wire to pass to its extremity, to draw the plug out if broken by any accident, also to admit the expansion of water, in case of severe frost.

The blocks or sleepers, E F G, on which the tram-plates are placed, should by no means be less than 120 pounds each in weight, but should be heavier on some kinds of ground: the depth of the hole for the plug should be three inches, and worked according to the inclination of the plug, for which purpose the stone-mason should have a standard cast-iron gauge; there should be projections, K, cast with the flanch or outside edge of the tram-plate, as shown at fig. 1, to make the plates lie firm on their sleepers.

Fig. 4 is a section of one of the ends of a tram-plate, in which H shows the flanch or upright edge, I the flat part or sole on which the wheels of the waggons run, D one of the plugs, K the projection behind the flanch to make the plate lie firm on the blocks.

GENERAL OBSERVATIONS.

Advantages of this compared with the common mode.

The advantages of laying plates on the above principle are obvious; the blocks being put in their places never sink below their intended level, the act of driving either nail or

or plug, (which requires a considerable degree of force, and too frequently destroys the level of the road) being here unnecessary. In the common mode of making rail-roads, from the irregularity of nails, particularly in forming their heads, few can be driven exactly even with the plate, and they are perpetually obstructing the passage of the waggon; the workmen frequently not proportioning their holes and plugs to the hole in the block also occasions considerable breakage; the exertion necessary to fix a rail or plate completely is great, and numbers of plates, particularly when the iron is short or brittle, are broken near the mortices by missing the stroke of the hammer, which must be used with great force.

Advantage gained in laying my Tram-Plates in Comparison with other Modes

	£.	s.	d.
Nails used in a mile, 3520 of 3 in the pound, at 4*d.* per lb. - - - - -	19	11	0
Nails lost or defective, computed at per mile	1	0	0
Plugs with their loss - - - - -	6	5	0
By breakage of rails, average from experience	7	10	0
Lessened by labour in block laying, calculated at only two pence per yard - - -	14	13	4
By breakage of blocks - - - -	1	0	0
	£49	19	4

Saving in expense.

This calculation does not take in annual loss of nails, and breakage of blocks, which is considerable.

VIII. *Observations*

VIII.

Observations on the Use of Acid Fumigations in purifying the Air and stopping the Progress of Contagion, and the most simple Means of completely obtaining this Effect. Extracted from the Correspondence of Mr. GUYTON-MORVEAU *.

Contagious disease stopped by acid fumigations.

I CANNOT yet give you any particulars of the disease, that prevailed at Pethiviers at the end of last summer; though several persons, who have seen the reports of the officers of health, have assured me, as I informed you at the time, that acid fumigations had evidently put an end to it. I shall say nothing of what may have prevented the result from being published, or why it has not even been mentioned in the newspapers. You will scarcely believe, that indifference could be carried so far on a subject of such importance. No doubt it would be otherwise, if government appeared to set any value on the discovery: but there are so many petty interests, that oppose its utility being made known; and it is so much more easy for a man to throw a veil over the efficacy of the means, than to confess himself chargeable of having been ignorant of them, or of having neglected to apply them to the benefit of mankind!

Obstructions to improvements.

Infectious diseases arise without direct contagion.

I have been informed, that a physician in considerable repute asserted a few days ago, that these fumigations were inefficacious against diseases not communicated by infection. It would not have been amiss to have asked him, whether he thought balls and bayonets conveyed any infectious principles: or whether he did not know, that, whenever a great number of wounded persons were conveyed to an hospital, a disease did not soon break out, that attacked the nurses, the medical attendants, and often a whole town. For, as I have said on a former occasion, whatever be the first cause or the nature of a disease, that affects several individuals at the same time, it necessarily produces an hospital fever, which ultimately occasions more ravage than the original disorder; and it is precisely against this scourge, that mine-

Hospital fever.

* Annales de Chimie, vol. xlvi, p. 114.

ral

ral acid fumigations afford a certain preservative, a remedy the efficacy of which can no longer be disputed.

The more secret motives there are, to mislead opinion on this subject, the more incumbent on me it is, to collect such facts as testimonies, as may produce conviction. I have at present some to communicate to you, which cannot fail to appear capable of making an impression on all those, who are not misled by prejudice; and I will add some remarks on the bottles for destroying contagion, which are not yet sufficiently known, and the method of employing them for the use of a large hospital, so as to have always at hand, without trouble or expense, a certain preservative, and a powerful stimulus, the strength of which may be regulated at pleasure. Testimonies collected by Mr. Guyton.

Letter from Dr. Mojon.

Dr. Mojon, professor of chemistry and member of the Medical Society at Genoa, who had already furnished me with some valuable information respecting the use of mineral acid fumigations in that city, which makes a part of the additions to the second edition of my treatise, has since addressed to me the following letter. At Genoa acid fumigations employed against

" As soon as we perceived the progress of the epidemic fever, recourse was had to the acid fumigations in our churches, hospitals, lazarettoes, prisons, and barracks, and in the chambers of several sick persons. an epidemic fever,

" On the 20th of March, 1800, I was sent for to St. Andrew's church, where two grave-diggers had dropped dead, just as they were going down into a vault. I found the church offensive with putrid exhalations; and immediately directed the vault to be walled up. After causing the windows to be shut, I placed in the midst of the church a large earthen vessel containing six pounds of common salt, and three of sulphuric acid. Some lighted faggots were placed round the vessel, to accelerate the extrication of the vapours, which ceased at the expiration of two hours, and then the windows were opened. The noisome smell was completely gone, and the church was used as before, without any thing offensive being perceived. in a church against noxious effluvia,

" I ob-

in a very large church,

"I observed the same effect still more strikingly, when I employed oxigenized muriatic acid fumigations in the largest edifices, particularly in St. Dominic's church, where the air was so noisome, and so loaded with putrid emanations, that its stench was perceptible to some distance and in the adjacent houses. For this fumigation I employed eight pounds of salt, four pounds of sulphuric acid, and a pound and half of black oxide of manganese.

and in the chambers of the sick.

"To purify the air of close and habited houses, I preferred fumigations with nitric acid, which were equally successful, destroying the contagious miasmata without occasioning the least inconvenience to the sick. There is no instance of any one having caught infection from a sick person, where these fumigations were constantly used.

Acetic acid used as a preservative.

"To secure myself from the effects of the putrid and contagious exhalations, to which I was daily exposed, I made use of no other prophylactic than a small phial of acetic acid (radical vinegar), which I held to my nose, and by its means I had the happiness of escaping the infection during the whole continuance of the epidemic."

Effects of Fumigations with oxigenized Muriatic Acid, as reported by Mr. Fleury, Officer of Health in the Navy.

Oximuriatic acid fumigations in dysentery,

Mr. J. A. Fleury, in his Essay on the Dysentery, presented to the School of Physic, January the 4th, 1803, after classing frequent fumigations with oxigenized muriatic acid among the general means of regimen and renovation of the air, adds what follows as a note in support of this precept.

and against the hospital sore.

"After the signature of the preliminaries of peace and the treaty of Amiens, a great number of French prisoners landed at Cherbourg. The sick were carried to the naval hospital, of which I had the charge. Several were afflicted with the ulcer called the hospital sore, to remedy which all the resources of physic and surgery were employed in vain. The fumigations of oxigenized muriatic acid, employed habitually to correct the air, were particularly directed to these ulcers; and soon the contagion, which imparted to other ulcers and recent wounds the same character, was stopped,

stopped, and I had the satisfaction to see cures gradually effected, which I had before attempted in vain. Though these fumigations were made in wards full of patients, from the impossibility of avoiding it, and this rendered their more frequent employment necessary, I never found one person complain of them, or sensibly incommoded by them. It was not the same with those, who entered when the vapour was already in a state of expansion; as it brought on these a violent cough, and obliged them to quit the ward immediately."

On the Preparatiom and Use of the Phials for removing Contagion.

The phials for preventing and removing contagion, which I have mentioned in my treatise, have become pretty common, since they have been kept ready prepared in the shop of Mr. Boullay. In fact it is not easy to conceive any thing more simple in its preparation, more convenient for use, and less expensive, considering the property this composition has of retaining its virtue a long time. I have one of these phials prepared near twelve years ago, and it cannot now be uncorked, without the persons present being immediately sensible of the oxigenized muriatic acid gas, though I have used it on various occasions, and no addition has been made to it since it was first prepared. This is the property of what I call *extemporaneous* oxigenized muriatic acid, because it is made in an instant, without the aid of fire or any apparatus for distillation, in short by simple mixture.

Phials for preventing and removing contagion.

However easy this process is, certain proportions are nevertheless necessary to be observed, in order that the evolution of the gas may be sufficiently abundant to be efficacious, yet not so rapid as to burst the vessel: and it is obvious, that the proportions do not depend simply on the relative quantities of the acids in this case, but still much more on their degree of concentration.

The ingredients must be accurately proportioned.

Several persons, having seen the stopples of these phials rise spontaneously the moment they opened them, were alarmed, and mentioned it to Mr. Boulay; who accordingly thought it necessary to dilute the mixture, that it might be less

less active. Soon after this I was informed, that phials recently bought of him scarcely made any impression on the nose when opened under it; and some were brought me, from which I found in fact but a slight smell of common muriatic acid. On my representing this immediately to Mr. Boulay, he at once made his phials as strong as they ought to be; and I know, that those he has since sold were well prepared. This however suggested to me, that it might be proper, to determine the process with more precision than I had hitherto done, I mean with regard to weight and measure, in order to have a liquid at all times manageable, and of a permanent degree of activity. I shall proceed therefore to give the receipt with all its particulars.

The phials described.

The phials to be made *portable* should scarcely exceed in capacity $4\frac{1}{2}$ centilitres [$1\frac{1}{2}$ oz.], or 45 cubic centimetres [about $2\frac{1}{4}$ cubic inches]. This is the measure of those prepared by Mr. Boulay, who puts them into a case of hard wood, commonly of box, neatly made. The cap of this case screws on. It is unnecessary to say, that the stopple of the phial must be of glass, and ground to fit perfectly tight.

Method of preparing them

Having selected a phial, it is to be measured. Suppose its capacity to be just 45 cub. cent. [$1\frac{1}{2}$ oz.], 3 gr. [46·32 grains] of black oxide of manganese are to be put into it, powdered, but not finely, and sifted only through a hair sieve.

To these add 7·5 cub. centim. [about $\frac{1}{5}$ of a cubic inch] or $\frac{3}{4}$ of a centilitre [$\frac{1}{4}$ of an oz.] of pure nitric acid of the specific gravity of 1·4 (about 39° of Baumé's areometer), and an equal measure of muriatic acid of the specific gravity of 1·134 (about 17° of Baumé's areometer).

The stopple being put in, the process is finished.

They must not be more than one third full.

It is to be observed, that about two thirds of the capacity of the phial will thus remain empty. This is an essential condition, without which it is impossible to confine the gas. Having once exceeded this proportion in a very strong flint-glass phial, that would hold 4 decil. [13 oz.], I saw the stopple, which weighed 122 gram. [1884 grs.], driven out to such a height, that in falling it broke the phial. But it is easy to prevent all accidents, by keeping within the limits here assigned.

It

It will not be improper, to add to the instructions for preparing these phials the manner of using them; for things acquire value only by the skill with which they are used.

In the first place it is to be observed, that the intention of the screw is chiefly to bring the cap of the case to the proper point for confining the stopple of the phial; which might otherwise be raised up by the expansion of the vapour, and allow the acid to escape; so that, if you were to endeavour to turn the screw as far as it would go, or even if you were to turn it with too much force, you would necessarily split the cap, or even crack the neck of the phial, which would be still more dangerous, if it were not immediately perceived. Manner of using them.

I have observed, that the first thing done by those who are unacquainted with the nature of acid gasses is, to apply the phial to the nose, as they would a smelling bottle; whence they feel an irritation so much the more painful, because it is more quickly carried to its maximum. It is necessary therefore, to inform such persons, that the phial for destroying contagion should not be brought near the nose: on the contrary, it should be kept at a distance from it when it is unstopped; and when it begins to make an impression on the olfactory nerves, it is time to stop it again, unless it be required to render the fumigation efficacious in a given space, as when the object is to purify a chamber rendered noisome by putrid effluvia. In this the phial should be placed on a table, and left unstopped for some minutes. With these precautions we may obtain all its good effects, without experiencing the least inconvenience. Caution.

So much for the use of the portable phials: you will soon see, that I have carried my views much farther respecting the advantages to be derived from the preparation they contain.

Permanent Apparatus for destroying Contagion in Hospitals, public Places, &c.

It is with some unwillingness I employ the word *apparatus*, which may perhaps be sufficient to frighten many persons, Permanent apparatus for large buildings.

sons, though in fact it is applied only to a vessel kept ready to be opened when necessary, and which in this view might be called a *box of salubrity;* but the name is of little importance, and I leave it to custom to settle this, so the thing itself be adopted.

The mixture continues to yield oximuriatic acid gas a long while.

In reciting the numerous experiments I made in one of our hottest summers on considerable bodies of air contaminated by sanious putrefaction, I announced, that I took the precaution frequently to leave open in my laboratory a very large phial, containing the mixture above mentioned for the extemporaneous production of oxigenized muriatic acid gas. This phial, which had since been neglected, having fallen in my way a few days ago, I was surprised, on taking out the stopple, at the strength of the gas it still furnished after the expiration of two years. This convinced me, that the mixture, kept in sufficient quantity in large vessels, would supply the place of all fumigations to destroy contagion, and answer the purpose as effectually, without trouble, expense, or inconvenience, and without its being necessary to renew the preparation till after a considerable time, even in cases where it would be most frequently necessary to give issue to the gas.

Necessary conditions.

It is obvious, that the capacity of the vessel must be proportionate to the extent of the space to be purified, and its aperture sufficiently large, to give instantly the volume of gas required; that is to say, so as to extend to every part, without too powerfully affecting those who are nearest to it. Lastly, the gas must be so confined, that it cannot burst out, or even escape imperceptibly: in a word, so that it will only diffuse itself around when we wish it, cease to be diffused at pleasure, and remain for months without its presence being suspected.

Easily obtainable.

All these conditions are easily obtainable for the largest hospital ward by the means I am about to describe.

Apparatus described.

Take one of those very thick flint glass tumbers, which are common in the shops, of 11 or 12 cent. [4·3 or 4·7 inches] high, and 10 [3·9 in.] in diameter; holding about 7 decil. [1·47 wine pint]. Grind the edge so that it may be closed by a piece of plate glass. Cement the bottom of the tumbler into a piece of wood, which is to be made fast by

by sliding horizontally into grooves at the bottom of two uprights. To these uprights a cap must be fixed, through which passes a screw, to raise or lower the stopple by means of a nut in a sliding cap, to which the piece of plate glass is to be cemented.

An inspection of the figures, *Pl.* X, *Figs.* 5 and 6, will show the form and dimensions of every part of this little apparatus, which should be wholly of wood, without iron or any other metal, and the construction of which requires nothing extraordinary or at all expensive.

A a small square of wood, into which are fixed the two uprights, B B.

C a glass tumbler, cemented into a little movable piece of wood, *d*, which is secured by a groove in each of the uprights.

E a wooden screw, passing through the upper cross piece F, and carrying the movable cross piece G, which slides on the two uprights by means of a groove at each end.

H the plate of glass performing the office of a stopple, and cemented to the under side of the movable cross piece.

The vessel being thus arranged, and its capacity being we will suppose 7 decil. [near 1½ pint], pour in first a decilitre [3·369 oz.] of nitric acid, of the strength mentioned above, and then an equal quantity of muriatic acid; add 40 gram. [618 grs.] of black oxide of manganese powdered; and immediately close the vessel by screwing down the stopple. These proportions are assigned from the necessity of leaving at least two thirds of the capacity of the tumbler empty.

If the contagion were considerable, or if the foci from which it issued were sufficiently numerous, to renew it in a short time, it would be advisable to have two or three such apparatuses in the length of the ward.

In large or very foul places two or more necessary.

In a place less extensive than I have hitherto supposed, in a ward for instance containing but ten or twelve beds, or in a public room where the air is vitiated only by a temporary accumulation of animal effluvia, we might employ, instead of one of these tumblers, a wide mouthed stopple bottle, such as the glassmen sell for the use of chemists. Their

In some cases a large wide mouthed phial sufficient.

Their capacity is commonly about 40 or 45 centil. [14 or 15 oz.]; and their stopples, which are made to fit tight, are 3 cent. [1·18 inch] or more in diameter.

It is obvious, that by putting into one of these phials 6 centil. [2·22 oz.] of each of the two acids, and 24 gram. [370 grs.] of black oxide of manganese, we shall readily obtain a reservoir of gas for destroying infection. The only thing to be apprehended, against which the apparatus above described is secure, is that the stopple, being secured only by its weight and the friction of the neck, should be forced out by the elasticity of the gas: but this may be prevented by loading the stopple with a heavy cap of lead.

Method of using them.

No farther direction need be given respecting the mode of using these reservoirs of gas for destroying contagion, than to open them when it is deemed requisite, and to close them as soon as the persons nearest at hand begin to be affected by them. After this we may rely on the spontaneous expansion of the portion set at liberty. The effect will be such, that, if the vessel have remained open only four or five minutes, those who come into the room an hour after by the remotest door will immediately perceive, that oxigenized muriatic acid gas has been extricated.

Advantages of the oximuriatic acid.

You will be of opinion assuredly, that of all the processes adopted for fumigation this is the most simple, the least liable to accident, and the best adapted to common use: and when you consider, that the gas it sets in action is acknowledged to be the most efficacious, even by those who have appeared to dread its activity, because they were unacquainted with the means of moderating it at will; and lastly, if you adopt the decided opinion of several professional gentlemen, that this gas has the property of exciting the vital powers above all other acid gasses; you will perhaps think with me, that when the petty passions are exhausted by struggling against conviction, the oxigenized muriatic acid gas will be adopted in preference as the primary antidote to contagion, and its extemporaneous preparation will be the most common resource in the regimen of health.

IX. *Account*

IX.

Account of a Well for preserving and filtering Rain-water for domestic Purposes, where a Supply of Spring-water was not easily to be obtained. Communicated by J. R. Gowen, *Esq.*

To Mr. NICHOLSON,

SIR,

You may perhaps deem the following account of a filtering rain-water well, which has been successfully tried here by the Earl of Caernarvon, not undeserving of notice in your valuable Journal. His lordship has lately erected, upon a dry gravelly eminence in his park, an ornamental circular building, consisting of a room and open colonnades above, and apartments for cottagers upon the basement floor. Considerable discussion arose upon the mode of supplying them with water, from the great depth to which it was necessary to sink, in order to obtain an effective well. My friend, Mr. John Loat, builder, of Clapham, who had furnished the plan for the construction of the dome roof, mentioned to me a contrivance of his father's to meet a similar difficulty, which had been attended with invariable success, and Lord Caernarvon immediately determined upon carrying it into execution.

Well for filtrating rain water.

Difficulty of supplying water on an eminence;

Following Mr. Loat's instructions, we sunk two wells, 30 feet deep by 4 feet diameter each, which for greater perspicuity I shall call No. 1 and 2. They are a trifling distance asunder, and were carefully clayed, to prevent percolation into the surrounding soil, and lined with bricks in the usual manner. A well secured communication was made between the two wells, by a small leaden pipe inserted two feet from the bottom. All the pipes from the roof were directed into No. 1; and an oak floor, bored full of small holes, and supported upon posts, was laid in at No. 2, just above the pipe of communication. Upon this floor was first placed a stratum of well washed coarse gravel, then one of finer, next a stratum of coarse sand, and finally one

obviated by collecting rain-water in one well and filtrating it into another.

one of the finest sand we could procure, making altogether two feet in thickness of silicious substances. The water, which is received into No. 1, passes through the leaden pipe into No. 2, and filtrates by ascent through the strata of sand and gravel, the space below the level of the oak floor in both wells, acting as a cespool, receives all sediment. The pump is of course affixed in the filtering well. Both wells are covered up, but plenty of air is admitted to them, through apertures made for this purpose.

Advantages of this plan.

You will immediately perceive, that the merit of this plan consists altogether in the filtration by ascent, with a competent space under the apparatus. The interstices of the sand are thus never clogged, and its power is preserved unimpaired for an indefinite period. The well fully answers its intended purpose, and the water is altogether excellent. I have been tempted to submit this statement to you from a persuasion, that there are few houses, which may not be made in this manner to supply excellent water in sufficient quantity for domestic consumption; and that situations abound, where the filtrating well may be resorted to with equal comfort and advantage.

I am, Sir,

Your obedient humble Servant,

J. R. GOWEN.

Highclere, Newbury, Berks,
April 1, 1809.

X.

An Inquiry into the Structure of Seeds, and especially into the true Nature of that Part called by Gærtner the Vitellus. By James Edward Smith, *M.D. F.R.S. P.L.S.**

Gærtner first named and defined the *vitellus*.

GÆRTNER, so justly celebrated for his anatomical and physiological inquiries into the nature of seeds in general, and for his particular illustration of one thousand different kinds, claims the merit of first giving a name and definition

* Trans. of the Linnean Society, vol. xx, p. 204.

to

to a part called by him the *vitellus*, which, though not entirely unobserved by preceding philosophers, had received no particular description or explanation. Before we enter upon the investigation of this organ, it is necessary to consider the structure and functions of the parts of a seed in general; and this it will be best to do physiologically.

Germination of seeds requires moisture, heat, and air.

Three agents are necessary to the germination of seeds,—moisture, heat, and air. A seed committed to the ground absorbs, through the vessels of its base, the juices of the soil, or any other moisture that comes in its way; while it receives, throughout its whole substance, a definite portion of heat, some seeds requiring a greater share of the latter, for the purposes of vegetation, than others. Moisture and heat however are not of themselves sufficient to cause the germination of seeds. It has long been known, that air is equally necessary; and modern chemists have ascertained oxigen gas to be the particular ingredient of the atmospheric air which is requisite, and which is absorbed by seeds, in the moments of incipient germination, from or through the surrounding soil. Thus the bulk of the seed is increased, and its vital principle stimulated. It bursts its immediate integument, or *testa*, and in the first place sends forth the radicle, or young root, into the ground.

Integument.

Radicle.

This part being, as Dr. Darwin well observes, most susceptible of the stimulus of moisture, elongates itself in the direction in which it meets with this stimulus; and descending into the earth, while it fixes the infant plant, assumes its own proper function of imbibing nourishment for the future support of that plant.

Albumen.

But before any supplies can be thus obtained, considerable demands are made, even by the root itself; and not only an evolution of parts, but likewise an increase of bulk, takes place in the young vegetable. For this necessary purpose a store is prepared in the *albumen*, a substance either constituting a separate body by itself, as in grasses, corn, palms, &c., which, from a hard, dry, and tasteless mass, changes, by the action of water and oxigen, into a milky or saccharine fluid; or the same substance is lodged in, or united with, the bulk of another part, next to be mentioned, the cotyledon, or, as they are generally of the plural number, cotyledons.

Cotyledons.

As the root is the part stimulated by moisture, the cotyledons appear to be most stimulated by air, and they consequently raise themselves, for the most part, out of the ground in order to receive it, in the form of seminal leaves, well known to perform, for a time, the functions of real leaves, and even, by the action of light, to assume their green colour. The *albumen* cannot be said to be stimulated, or acted upon as a living body, by the air or gas, which only produces chemical changes in it; and the destination of this substance being soon accomplished, it disappears by absorption. Not so the other parts of the seed, one of which becomes the still descending root, the other the nurse, or, if we may say so, the foster brother of the young ascending plant, which last originates from the extremity of the embryo opposite to the root, but always, like that, most intimately connected with the cotyledons. These indeed, sooner or later, wither away; when the acquisition of real and more ample foliage renders them superfluous, or no longer necessary. But all cotyledons do not ascend out of the earth, nor assume any of those functions of leaves in which light is concerned. In the horse chesnut, the *cyamus nelumbo*, the *tropæolum majus*, and some other plants, they always remain buried, no doubt acted upon by the air or gas alone. Even in plants of the same natural order, *papilionaceæ*, some, as *lupinus*, raise their cotyledons into the air and light, in the form of very conspicuous green seed-leaves; while others, as *lathyrus*, retain them under ground, concealed in the black skin of the seed, quite out of the reach of every ray of the latter. In these we know a farinaceous *albumen* is lodged, whether they rise into the light or not; and the closest analogy leads us to conclude, that their functions are otherwise similar, which can only be with respect to air.

Not indispensable.

Even cotyledons however are not indispensably requisite to a seed, though the albumen appears to be, in some form or other, necessary to all seeds. Not to mention the tribes of vegetables allowed or guessed to be without cotyledons, and thence, for systematical convenience, denominated acotyledonous; all, who have sufficiently considered the matter, know that in those called monocotyledonous, what is vulgarly taken for

Absent in plants called monocotyledonous.

for the cotyledon is really an albumen, a part fundamentally distinct in functions from what is proper to a cotyledon. Thus even so conspicuous a family of plants as the *orchideæ*, which the faithful Jussieu confesses were only presumed from analogy to be monocotyledonous, or, as he guardedly expresses it, to have "a single-lobed *corculum*," have been shown by Mr. Salisbury, in the eighth volume of our Transactions, the only person I believe who has well examined their germination, to have in fact an albumen, but no cotyledon at all. Nor does such ambiguity or uncertainty belong to this family alone. Many plants are presumed to be monocotyledonous, chiefly because they grow in the water; and it is much to be regretted, that this fundamental principle of all natural systems should in many cases be so ill-established, and very often so extremely difficult to detect or to determine; which happens in general where its help is most wanted, as I shall presently endeavour to show; but I must first speak of the more immediate object of the present essay.

Gærtner asserts the *vitellus* of seeds to be "distinct from "the cotyledons as well as from the albumen, and, for the "most part, situate between the latter and the embryo." He considers as its principal diagnostics the three following characters: "1st, that it is most closely connected with the "embryo, so as not to be separable from it without injury "to its own substance: 2dly, that notwithstanding this in- "timate connection, it never rises out of the integuments "of the seed, as the cotyledons usually do, in germination, "so as to become a seminal leaf, but, rather like the al- "bumen, its whole substance is destroyed by the seedling "plant, and converted into its own nourishment: and 3dly, "that if the albumen be likewise present, the *vitellus* is al- "ways situate betwixt that and the embryo, in such a man- "ner, however, that it may be separated from the albumen "with great ease and without injury." For which reasons this able writer considers the organ in question as "allied "on the one hand to the albumen, on the other to the co- "tyledons," but truly distinct in nature from both. He proceeds to observe, that "it is of all the internal parts of "a seed the most singular, and by far the most unfre- "quent."

Vitellus.

Its characters according to Gærtner.

Now,

These do not hold.

Now, to consider all these points separately, in the 1st place the *vitellus* is not more closely connected with the embryo than the greater part of cotyledons are; according to the figures and descriptions of Gærtner himself, the fidelity of which must be evident to any one in the habit of using his book, and especially to those who will take the trouble of comparing a few of them with the seeds to which they refer, while in the earliest stage of germination, at which time the relative connection of the parts is best ascertained. 2dly, That the *vitellus* never rises out of the ground, is a circumstance common to it with many cotyledons, allowed to be such by Gærtner, as in the leguminous plants, and others already mentioned. 3dly, That the *vitellus* is situate between the albumen (if the latter be present as a separate organ) and the embryo, is only a necessary consequence of the more intimate connection between it and the latter than either of them has with any other part, which is also precisely true of the cotyledons and embryo, as above mentioned.

It does not differ from the subterraneous cotyledons.

For these reasons I presume the *vitellus* to differ in no respect from the subterraneous cotyledons already described; and that its office is to perform the necessary functions relative to air or oxigen, till the leaves come forth and assume those functions, in greater perfection, with the cooperation of light. This seems more satisfactory than the opinion of Gærtner, that the organ under consideration affords nourishment to the embryo; because this is abundantly supplied by the copious *albumen* of a multitude of seeds, the *vitellus* of which is very inconsiderable, as grasses; and because it is unphilosophical to recur to two causes, when one is evidently sufficient. In fact, the *vitellus*, as far as I can observe, only dwindles away when the leaves unfold, exactly as happens to the subterraneous cotyledons. The same thing very often takes place as speedily in those which rise out of the ground; the existence of the latter appearing to be prolonged in some instances, merely by their nearer approach to the nature of leaves, as in umbelliferous and cruciform plants. The difference of duration is still more evident, and more instructive as to our present purpose, in the leguminous family, between such cotyledons as rise above

above the ground, like lupines, and those which remain buried, like vetches, the latter decaying as quickly as any supposed *vitellus* can do. In grasses the scale, taken by Gærtner for a *vitellus*, is mostly so thin and unsubstantial, as not possibly to contain any material portion of nourishment; but its expanded figure is very well calculated, like that of the leaves, for functions analogous to vegetable respiration, and its whole aspect conveys the idea of a primary or subterraneous leaf, quickly rendered superfluous by the production of real leaves, which, as well as the radicle, are probably, in the first stage of their evolution, fed by the abundant juices of the albumen. It appears, that the pretended *vitellus* is not necessary to all plants furnished with this distinct kind of albumen. The palms and *orchideæ* prove to be destitute of it. On the other hand, I can find no instance of a supposed *vitellus*, and a real cotyledon or cotyledons, in the same plant. What Gærtner terms the cotyledons of *rhizophora*, in his *tab.* 45, appears to me to be the *plumula;* and in his descriptions of some of the *scitamineæ*, he evidently takes the latter for a cotyledon.

The scale of grasses analogous to leaves in its functions.

Vitellus never present with a cotyledon.

By understanding the *vitellus* as a cotyledon, all ambiguity respecting the component parts of any seed is removed. When the cotyledons are two or more, the only question is, whether the albuminous matter is lodged in their substance, or whether it forms a separate organ. When the embryo is accompanied by a simple undivided organ or seed-lobe, we know it to be a cotyledon by its strict union, or even partial incorporation, with the embryo, as in *zamia**; whereas the pure separate albumen of the true palms has, as in every other instance, no more connection with the embryo, according to Gærtner's just remark, than is absolutely necessary: and moreover evinces its true nature by the chemical alteration, and speedy absorption, of its whole substance. The cotyledon, as I consider it, of *zamia*, as in

Considered as a cotyledon.

* Mr. R. Brown, who has observed the germination of a large species of *zamia* in New Holland, assures me that he found no such incorporation of the parts in question as Gærtner has represented in his *t.* 3, and that the structure and evolution of every part bore an exact resemblance to *cycas* as described by Mr. Aubert du Petit Thouars.

numerous

numerous parallel instances, shrivels and shrinks indeed considerably, from the absorption of its albuminous contents by the vegetating embryo, but does not disappear, leaving only a skin behind, like the albumen of grasses or corn, because that part of its substance, which is destined to perform the office, essential to a cotyledon, concerning air, merely decays when its end is answered.

Difference in the albumen.

It may further be observed upon this subject, that the albuminous matter of seeds with two or more cotyledons is commonly of an oily nature, while those with one cotyledon, or none at all, have a more farinaceous, or even stony, albumen. Still the latter changes to a milky or oily fluid, previous to its absorption. When the vital principle of a seed is extinct, its albuminous oil becomes rancid, and, even in seeds that retain life, is liable to suffer some deterioration by keeping. Hence, as Darwin observes, gardeners preserve melon and cucumber seeds, perhaps for years, that the plants they produce may be less luxuriant, in consequence of being starved at their first germination; for any injury to the cotyledons, even after they begin to rise above ground, is found to cramp the subsequent growth of the plant.

Oil of the cotyledons.

The oil of the cotyledons has been usually supposed a protection to their internal parts, I presume against wet; but this purpose it by no means does or can answer, for all seeds readily absorb moisture whenever they meet with it, and, if likewise exposed to the action of oxigen, they vegetate, in whatever situation they may otherwise happen to be. I suspect moreover that the oily and mucilaginous fluids of seeds in general, before they perform their office in germination, all previously become milky, and often saccharine, from the actions of water and oxigen. It might be worth while to inquire, whether exposure of such seeds as are most prone to turn rancid, to a quantity of oxigen, would tend to preserve them. It is, I believe, found, that the admission of some atmospheric air is necessary to the preservation of many seeds. The primary cause of decay therefore in seeds spoiled by keeping may originate, not, as I have supposed, in the extinction of their vital principle, but in the corruption of their albuminous oils; and this is

Would oxigen preserve seeds prone to turn rancid?

strengthened

strengthened by the experiments of the French chemists, whose applications may much more readily be supposed to correct and restore the albuminous juices, than to bring the dead to life.

This idea of the albuminous matter, whether oily, mucilaginous, or farinaceous, being, when not a distinct and separate body, always lodged in the cotyledons, throws additional light on the nature of the last mentioned parts, and in a very beautiful manner confirms their analogy with leaves. The discoveries of Mr. Knight have proved, that the nutritious fluid or sap of plants is carried into the leaves, in order to be there acted upon by air, light, heat, and moisture. After these agents have produced their effects, the fluids are sent back, through the returning vessels, into the branch or stem, to furnish matter of increase to the whole vegetable body. The chemical experiments, of Dr. Priestley more especially, confirm this, by teaching us, that carbonic acid gas is absorbed by leaves in the day time through their upper surface, and decomposed by them, its carbon being added to the sap, and its oxigen emitted by the under surface. In the dark, leaves are found to absorb oxigen. Let us apply all this to the germination of seeds. The oxigen, known, as I have already said, to be necessary to this process, being conveyed to the seed in its dark subterraneous situation, is absorbed by its cotyledons, already stored, from the constitution of the parent plant during their formation, with albuminous matter abounding with the carbonic principle. The chemical action of the oxigen on this albuminous substance renders the latter a more or less saccharine, and, with the addition of the imbibed moisture, a milky fluid, fit to be transmitted, through the returning vessels of the cotyledons, into the stem of the embryo, especially as all these important parts have already begun to swell by the absorption of moisture assisted by warmth. Hence we see why light is found hurtful to incipient germination, and why carbonic acid gas may be given out by seeds at that period. We perceive also why the outside of seeds is so commonly dark coloured, or even black, as in *canna*, *afzelia*, and others, it being the only part of the vegetable body, as far as I recollect, that is ever positively black, except perhaps

The albumen, when not a separate body, lodged in the cotyledons.

Chemical process in germination.

Light hurtful to incipient germination.

haps the skins of some fruits. It is, moreover, evident, that all the indispensable functions of the cotyledons are best performed under ground, and that when they rise into the air and light, it is not till after their primary destination is fulfilled, and then because, being fundamentally of the nature of leaves, they are also capable, in most instances, of assuming their functions with respect to light. It is highly worthy of notice, that, in consequence of the original position of the cotyledons in all seeds, the oxigen gas must always be imbibed by their under side, that very same part which in leaves gives out this kind of gas during the day, and probably absorbs it during the night. It would have evinced a strange contrariety in the constitutions of two organs otherwise so analogous, I mean the leaves and cotyledons, if the *upper* surface of the latter, while in the unexpanded seed, had been presented to receive the oxigen gas.

Stalk of the embryo may perform the function of a cotyledon, where the latter is wanting.

Where there is a separate albumen, without any perceptible cotyledons, it is probable that the stalk of the embryo may answer the necessary purpose; just as the stems of leafless plants must be presumed to perform the usual chemical functions of leaves, though we cannot ascertain in what direction the different airs are imbibed or discharged, there being no decided upper or under surface in such stems, any more than in ensiform leaves. Such, however, are rare exceptions, which if not, as yet, found to throw any new light on the subject, certainly do not overturn any important part of the above hypothesis. That some part, immediately connected with the embryo, must be stimulated in order to excite the germination of a seed, this phænomenon being dependent on the vital principle, is evident. I conceive that, when present, the cotyledon or cotyledons are themselves stimulated by the oxigen gas, or rather by the heat which chemists inform us is produced by the absorption of this gas, so as to set their fluids in motion, and thus to propel the young root and rising *plumula*. But when the cotyledons are wanting, the embryo may very well be conceived capable of sufficient action, to imbibe for itself the juices of a distinct albumen, already become milky and saccharine by the reception of oxigen and moisture; by which merely chemical process, as in barley, so considerable a degree

gree of heat is evolved, as must very powerfully excite the vital principle of the budding vegetable. In the few cases where one or more cotyledons and a distinct albumen are together present, it does not seem necessary, that the gas should act through the former upon the albumen, the two organs being but little connected, and its operation on the latter being independent of all vital or organic laws; but either the gas itself, or the heat produced, may very well so stimulate the vital principle of the cotyledons, as to propel their fluids into the embryo, and assist germination. This opinion is the more probable, as these fluids must be supposed more truly of the nature of sap, and more immediately fit for the use of the infant plant, than the liquor of the albumen. However this may be, the existence of a cotyledon or cotyledons, together with a separate albumen, in seeds, seems to me so unusual, as not to occasion much difficulty, and I would define a cotyledon to be a vital organ, capable, as such, of being stimulated by oxigen, heat, or both, for the propulsion of its contents; while such an albumen is merely a repository of nutritious vegetable matter, subject to the laws of chemistry alone, and only passively resigning those contents to the absorbing powers of the embryo, to which it is attached.

Definitions of the cotyledon and albumen.

I must now, under the impression of what has just been advanced, return to the arrangement of plants by their cotyledons.

Arrangement of plants by their cotyledons.

Plants in general are dicotyledonous, having a pair of these organs, which commonly rise out of the ground; but if they do not, it appears, from the consideration of the leguminous tribe, that such a difference could scarcely serve for a generic distinction, much less for that of a class or order. It also appears, that, if the number of cotyledons exceeds two, as in *pinus* and a few other instances, the difference is of little or no use for systematical purposes, and of no physiological importance whatever. The cotyledons of *pinus* all present their backs to receive the oxigen.

Those with two or more.

Some plants appear to be really furnished with one simple cotyledon, as *zamia*, and according to Gærtner's figures and descriptions, the true *scitamineæ*, as *amomum* (his *zingiber*), *alpinia*, &c.; while *canna* seems to have no cotyledon, but only

Monocotyledonous.

only an albumen. Can this be true? and if so, what is the value of such a distinction in a natural classification? The *liliaceæ*, *palmæ*, and now the *orchideæ*, are acknowledged to be acotyledonous, having only an albumen; while the grasses, so nearly allied to them, have one cotyledon, for I presume their scale must be admitted as such. Gærtner's phrase of *embryo monocotyledoneus* applied to these last-mentioned families may occasion a mistake, which would be avoided by the term *embryo simplex*, or *indivisus*, expressing his idea of the simple figure appropriate to this part in such plants, but which does not prevent its upper extremity being strictly analogous to the *plumula* of the *dicotyledones*. It seems to me therefore, that this learned writer is mistaken in saying the monocotyledonous plants never have any *plumula*. They have not indeed that feather-like configuration in the ascending point of their embryo, which gave rise to the name, but the organ so called is, and must be, present. To dispute about the term is as little to the purpose as to contend, that the *orchideæ* have no *pollen*, because it is not of a powdery appearance.

Ferns.

From Mr. Lindsay's account of the germination of ferns in our 2d volume, this family must be deemed monocotyledonous. Their germination seems at first analogous to that of mosses, as given by Hedwig in his *Theoria*, but the numerous and branched cotyledons of the latter overset all analogy, and indeed all classification of plants by the number of the parts in question. Nothing could be more unnatural than to separate mosses for this reason from the other cryptogamic vegetables, and therefore Jussieu can scarcely believe these parts to be cotyledons; yet it is not possible to call them any thing else, and to suppose them a peculiar, and hitherto unheard of organ, would but increase the difficulty. Gærtner in the Introduction to his great work,

Fuci.

p. 157, tells us he has seen many cotyledons in several *fuci* also, and that he suspects others of the more imperfect plants, hitherto referred to the *monocotyledones*, may be similarly circumstanced. It seems that too much, by far, has been taken for granted in this department, though the parts under consideration form the great hinge upon which all natural sys ems turn. It is only by analogy, that the great family,

family, or natural order, of *lichenes* has been judged monocotyledonous, an analogy which the *fuci*, if Gærtner be correct, render very doubtful. The germination of the *fungi* is at least equally uncertain.

Lichens.

Fungi.

I mean not however by any means to invalidate the importance of the distinction between such plants as have two or more cotyledons, and such as have only one or none, however inaccurate the terms commonly used to distinguish them may be. Much less am I inclined to throw any needless impediments in the way of those, who labour at the arduous and important study of natural classification, or to detract from the well-earned fame of such men as Gærtner and Jussieu, on account of difficulties and imperfections unavoidable in so abstruse a study. No real friend to truth and knowledge ever foments invidious rivalships in philosophy. The field of science is now so vast, that its different cultivators find the advantage of dividing their tasks, and thus the students of physiology, of natural systems, and of artificial ones, may all powerfully assist each other. Truth is pursued by different paths, and nothing is more pleasing than to see the various observers of Nature, in a Society like ours, mutually and harmoniously contributing, as we have all along done, to enrich the scientific hive. I would therefore conclude by recommending those, who have leisure and opportunity for the purpose, to observe for themselves the germination of the principal families of plants, not only of such genera as are in dispute, but of all about which there can be any doubt, most of which will easily be indicated by a comparison of Gærtner's work with the remarks in the foregoing pages.

Cultivators of science should divide their tasks, and assist each other without rivalship.

Norwich, Nov. 2, 1807.

XI. *Observations*

XI.

Observations on Nauclea Gambir, the Plant producing the Drug called Gutta Gambeer, with Characters of two other Species. By WILLIAM HUNTER, *Esq. Secretary to the Asiatic Society. Communicated by the President**.

Gutta gambeer produced from a species of nauclea.

IT has been a question, among naturalists and writers on the materia medica, whether the little cakes or lozenges called *gutta gambeer* be prepared from the *mimosa catechu*, or the produce of a different plant †. This question, if not already determined, I am enabled to resolve by actual observation, having seen the substance made from a species of *nauclea*, of which I beg leave to offer the following description.

NAUCLEA GAMBIR.

Characters of the nauclea gambir.

Funis uncatus. Daun Gatta Gambir. *Rumph. Amb. t.* 5, 63, *t.* 34, *f.* 2.

Climbing. Branches round. Leaves ovate, pointed, smooth. Stipules two, lateral, caducous. Peduncles axillary, solitary, simple, jointed.

Stem shrubby, twining to a great height, covered with a rough brown bark.

Branches crowded, round, smooth; branchlets opposite, widely spreading.

Leaves opposite, petiolated, ovate, pointed, waving, widely spreading, smooth, below marked with transverse parallel veins.

Stipules at the bases of the branchlets and petioles, two, lateral, parabolical, sessile, widely spreading, smooth, caducous.

Peduncles axillary, solitary, round, straight, horizontal, much shorter than the leaves; jointed near the apex and bracteated: after the flowers have fallen, the lower joint persistent, recurved, forming a hooked spine.

* Trans. of the Linnean Soc. vol. IX, p. 218.

† Murray, Appar. Med. vol. II, p. 549.

Bracteas

Bracteas four, ovate, acute, spreading, very small, caducous.

Flowers aggregate, globular; composed of very numerous florets, crowded on a globular, naked, very small receptacle.

Cal. Perianthium common, none.

Proper, one-leafed, oblong, incrusting the germen, persistent; mouth five-cleft, divisions lanceolate, erect.

Cor. as in the Genus.

Stam. Filaments five, very short. Anthers oblong.

Capsule stalked, oblong, incrusted and crowned with the calyx; tapering to a point below; two-celled, two-valved; the valves adhering at the apex, splitting at the sides.

Seeds very numerous, oblong, very small, compressed, furnished at both ends with a membranous pappus.

The flowers, when fully spread, I suppose last a very short time; for although I have frequently looked for them, I was never able to find them, whence I have been obliged to omit the description of the pistil.

Method of preparing the gutta gambeer.

From the leaves of this shrub is prepared the substance called *gutta gambeer*, in two ways. The first is by boiling the leaves *. This process was performed under my inspection, by a Chinese, at *Prince of Wales's Island.* Seven catties (or $9\frac{1}{3}$ lbs.) of the leaves, plucked clean from the stalks, were boiled in a large pot for one hour and a half, adding more water as the first wasted, till towards the end of the process, when it was inspissated to the consistence of a very thin sirup. When taken off the fire, and allowed to cool, it became solid. It was then cut into little squares, which were dried in the sun, turning them frequently. After one month, I weighed them, and found ten ounces and two drachms, troy weight.

Another mode of preparing it.

The *gambeer*, prepared according to this process, is of a brown colour; but from some parts of the *Malay* coast, and of *Sumatra*, it is brought in little round cakes almost per-

* See Marsden's Sumatra, p. 243,—where he quotes, for a particular account of the manufacture, the second volume of the Transactions of the Batavian Society.

fectly

fectly white. According to Dr. Campbell of *Bencoolen*, this is made by cutting small the leaves and young twigs, and infusing them in water for some hours, when a fæcula is deposited, which is inspissated by the heat of the sun, and moulded into round cakes.

Its qualities and use.

The *gambeer*, when first tasted, impresses on the palate a strong sensation of bitterness and astringency. But it afterward leaves a sweetish taste, which remains a long time. From these sensible qualities, it might reasonably be expected to prove useful in medicine. And accordingly, we are told that it has been found beneficial in angina and aphthæ, as well as in diarrhœa and dysentery. The drug was infused in water, to which it gave the colour of the infusion of bohea tea*. By the Malays it is mixed with lime, and applied externally to cuts, burns, boils, &c.

Chewed.

But the most frequent use of it is to chew, along with the leaves of betel, in the same manner as the *kut* (or catechu) in other parts of India.

Used in tanning and dyeing.

For this purpose the finest and whitest kind is selected. The red, being strong tasted and rank, is exported to *China* and *Batavia*, where it is used for the purposes of tanning and dyeing. For the first of these uses we might suppose, from its sensible qualities, that it is well calculated; and some rough experiments, which I have made on it with animal gluten, compared with those of Dr. Roxburgh on *kut*, evince it to be richer in *tannin* than that substance.

Rich in tannin.

Differences.

The chief places of manufacture are *Malacca*, *Siak* and *Rhio;* and the process of boiling is most generally practised; insomuch that the generality of manufacturers there are ignorant of there being any other. The colour and other qualities, they allege, depend on the vessel and the skill or attention of the operator. Thus an old manufacturer, with Chinese iron pots, will produce a whitish drug; whereas with a Malay iron pot its colour will be browner. The first cuttings also yield a whiter drug than the subsequent ones.

* Murray, l. c. Seba. item. Buisson apud Degner. de Dysent. p. 297.

For

For the cultivation of this plant a rich red soil is preferred. It gives the most luxuriant crop when the rains are frequent, but does not thrive in grounds that are apt to be flooded. On this account the side of a hill is esteemed better than any other situation. Culture.

The plants are propagated from seed. In three months after sowing, they appear above the ground; after this they grow fast, and may be moved to the field when nine inches high. They are there planted at the distance of eight or nine feet, so that one *orlong* (of eighty yards square) contains about seven hundred plants. At the end of one year Crops. from the time when they are planted in the field, a small crop of the leaves is obtained. A larger is got in eighteen months; and the third at the end of two years, when the bushes have attained their full growth. They continue in their prime, and admit of being cut twice a year, during a period of twenty or thirty years, provided care be taken to keep the ground clean and the roots free from weeds. Their tops must be cut so as to prevent them from growing to a greater height than five or six feet.

From good ground and a garden well kept, ten peculs Produce. (of $133\frac{1}{3}$ lbs. each) of dry gambeer are usually obtained on every orlong twice a year, or twenty peculs per annum. As it is cut every six months, and should then be boiled off, the leaves ought to be of the same age; but, from a want of means, it often happens, that the year is nearly expired before the cutting is done, which should have been made at the end of six months. In this case the young leaves yield a whiter drug than the old. As to the quantity afforded by each, in proportion to the weight of leaves, I have received contradictory information, so that I conclude little attention has been paid to this circumstance.

The price of the drug, at Prince of Wales's Island, va- Price. ries from four to eight Spanish dollars per pecul. The finest and whitest kind is that formed into little round cakes or lozenges. It is sold by tale, at three dollars and a half for the *laxa* (or 10,000), and one *laxa* weighs about 40 catties. This gives $8\frac{3}{4}$ dollars for a *pecul*.

The price of sago at Prince of Wales's Island is generally Adulterated about three dollars per pecul. Hence the manufacturer is with sago.

often tempted to adulterate his gambeer with this article, which mixes intimately, but may be detected by solution in water.

XII.

On the Variegation of Plants. In a Letter to Richard Anthony Salisbury, Esq. F.R.S. and L.S. by THOMAS ANDREW KNIGHT, *Esq. F.R.S. and L.S.**

MY DEAR SIR,

Variegated plants not attended to by naturalists.

THOUGH variegated plants have long occupied the care and attention of the gardener, it does not appear, that the peculiarities which distinguish them have much attracted the attention of the naturalist; and I am not acquainted with any experiments, which have been made either to discover the cause of variegation, or the effects produced by it. I am therefore induced to trouble you with an account of a few experiments, that I have made on one species of variegated plant, from which I obtained an unexpected and somewhat interesting result.

The variegated vine.

There is a kind of variegated vine, well known to gardeners, (the Aleppo), which affords variegated leaves and fruit; and as the grape, though small, possesses a very high flavour, and much richness, I wished to obtain some offspring either from its seeds or farina, with the hope of procuring berries of larger size, and at the same time of ascertaining whether its variegation would be transferred to the offspring.

Others impregnated with its farina became variegated.

With this object in view I extracted the immature stamina of the blossoms of the white chasselas, and white frontignac vines; and at the proper subsequent period I introduced the farina of the Aleppo vine: from this experiment I obtained, in the succeeding spring, many seedling plants. These plants, which were raised in a hot-bed, presented no singularity of character on their first appearance; but early in the succeeding summer I had the pleasure to observe

* Trans. of the Linnean Soc. vol. IX, p. 268.

purple

purple stripes in the seed-leaves of several of them; and in the autumn the leaves of many were variegated. I did not however obtain a single plant, which promised to produce, or has subsequently afforded, either coloured fruit, or coloured leaves, free from variegation.

Farina of black grapes does not render white ones variegated.

When, on the contrary, I have introduced the farina of a black, or purple grape into the blossom of a white one, none of the plants I obtained have ever been variegated; and the colour of the leaves and fruit, which these in the first year afforded, indicated with certainty the colour of all the produce of such varieties, in whatever soil cuttings taken from them were subsequently planted.

In variegated vines the effect different.

But in the variegated vines the result has been wholly different; and though the leaves and fruit first produced by some of them contained more tingeing matter than any of the coloured kinds, they subsequently produced, even on the same tree, some bunches almost entirely black, others perfectly white, others lead-coloured with stripes of white, and others white with minute black stripes; and grapes of all the preceding colours are very frequently seen on the same cluster. The leaves are also subject to the same variations, and the colours in them are in some instances confined to the *upper*, in others to the *under* surface, and sometimes extend quite through; and both the leaves and fruit of some of the branches have become permanently colourless.

Their colouring matter combined in a peculiar way.

It appears therefore obvious, that the tingeing matter of variegated grapes, though probably not essentially different from that of others, is differently combined, and united to the plant; and as the variegated grape afforded offspring similar to itself, and none similar to other vines, which permanently afford coloured fruit, it may be confidently inferred, that the nature of the union between the tingeing matter and the plants is very essentially different.

The variegated plants hardy and vigorous, and therefore not owing to debility.

All the variegated plants, that I obtained from the farina of the Aleppo vine, are not only perfectly free from disease and debility of every kind, but many of them possess a more than ordinary degree of hardiness and vigour: and two of them appear much more capable of affording mature fruit, in the climate of England, than any now

 cultivated.

cultivated. It is therefore sufficiently evident, that the kind of variegation which I have described is neither the offspring off, nor connected with, disease or debility of any kind.

Variegated cabbage.

But the same inference must not be drawn respecting other variegated plants; for variegation itself appears to consist of several distinct kinds. The leaves of a variety of the common cabbage are often seen, in the cottage garden, curiously tinged with different shades of red and purple, like the leaves of the vines which I have described: but in the cabbage these colours combine and melt into each other, whereas in the vines the distinct colours are separated by well defined lines. The colours of the cabbage are transferred to its offspring, which is perfectly hardy and vigorous.

Spotted lettuce.

The spotted lettuce must also be classed with variegated plants, and the offspring of this is as hardy as those of other varieties: but the most common kind of variegation,

Leaves striped with white and yellow apparently connected with debility.

in which the leaves are variously striped with white and yellow, though not the offspring, as some writers have imagined, of disease, is, however, closely connected with some degree of debility; possibly owing to the imperfect action of light on all such parts of the leaves as are either white or yellow. For I have observed, that variegated hollies are less patient of shade, than such as are wholly green; and I have never seen any plants, the leaves of which are wholly white or yellow, that continued to live

Leaves wholly white or yellow the same.

beyond a single season. A variegated plant of the raspberry, which sprang from seed in my garden, became wholly white in the third year; but it perished in the succeeding winter, and I should be disposed to conclude, that plants the leaves of which are entirely white or yellow, cannot long survive; but that du Hamel* has described a variety of the peach tree, of which he says, "son bois, ses feuilles, ses fleurs, et son fruit, tant extérieurement qu'intériurement, sont tout à fait blancs." This variety is at present, I believe, wholly unknown to our gardeners; and I suspect, that it was always a debilitated plant, and that it in consequence exists no more. I am, &c.

THOMAS ANDREW KNIGHT.

* In his Treatise on Trees.—*Article* Peach Tree.

XIII. *Method*

XIII.

Method of painting Linen Cloth with Oil Colours, so as to be more pliant, durable, and longer impervious to Water, than in the usual Mode. By Mr. William Anderson, *of his Majesty's Dockyard, Portsmouth* *.

SIR,

I BEG leave to lay before the Society of Arts &c. the following improvements and observations, which I hope will be of service to the public.

Having never heard or read of any method being discovered to prevent paint when laid on canvas from hardening to such a degree as to crack and eventually to break the canvas, and render it unserviceable in a short time; and having been an eye-witness for many years of much canvas perishing for want of such discovery in the immense quantities painted for covering seamen's hammocks, and for other uses on board his majesty's ships; I long had it under consideration to find out such an ingredient as, when mixed with paint, would preserve the canvas and paint laid thereon from the damages above mentioned, and after experiments for a considerable time, I have discovered such an article, and made trial of it with effect above three years.

Paint on canvas hardens and cracks it.

Ingredient to prevent this discovered.

The canvas I have painted has been submitted to the inspection of the Navy Board, who are so perfectly satisfied with my new method, that general directions are now given to paint all canvas in his majesty's dock-yards in this manner; which, in addition to the advantages I have before mentioned, actually saves an expense of one guinea in every hundred square yards of canvas so painted, as I have fully stated to them. The ingredient I use is not only serviceable for ship's canvas, but also for canvas designed for paintings, for floor cloths, and for painted coverings within and without doors. I have no doubt of it being applied to many other purposes I am yet unacquainted with; as from

Saves expense.

Answers for paintings, floor cloths, &c.

* Trans. of the Society of Arts, vol. xxvi, p. 136. The silver medal of the Society was voted to Mr. Anderson for this invention.

actual

actual trials of near four years, I can vouch for its being a preservative to red, yellow, and black paints, when ground in oil and put in casks. When the paints were examined at the expiration of such time, they discovered no improper hardness; but when laid on the work with a brush, they dried in a remarkable manner, without addition of any of the usual drying articles. I still preserve some of these paints for future trials, and I believe this plan of preserving colours will be of essential use to colourmen, and other persons who purchase colours for exportation. The ingredient I use is perfectly simple, being a solution of yellow soap; and the composition for painting is made in the following manner:

Preserves paint in casks for use.

The composition.

To one pound of soap I add six pints of water in a vessel over the fire; in a few minutes after the boiling of the water the soap will dissolve; while hot it is to be mixed with oil paint, prepared as hereafter directed, and is then fit for immediate use. The above quantity of soap solution will be sufficient to mix with one hundred weight of paint. The first coat to be laid upon the canvas is to be intirely of this composition, without first wetting the canvas in the usual way. A very small proportion of it, or none, is necessary in the second coat: and the third coat should be of oil paint alone.

Old method of painting canvas for sea use.

The method heretofore practised in his majesty's dock-yards for painting canvas, was as follows: The canvas was first wet with water, then primed with Spanish brown; a second coat given it of a chocolate colour, made from Spanish brown and black paint; and, lastly, finished with black. This mode is destructive, and more expensive than mine, in the proportion before mentioned.

New method.

In my method, to ninety-six pounds of English ochre ground in boiled oil, I add sixteen pounds of black paint, being one sixth in proportion of the ochre; this, when mixed, forms an indifferent black. The solution, made of one pound of soap and six pints of water, is to be added to this paint, and well united therewith; and without the canvas being previously wet, this composition is to be laid upon the canvas, as stiff as can conveniently be done with the brush, and this first coat will form a tolerably smooth surface.

surface. The second coat is to be formed of the same proportion of English ochre and black, without any soap solution; and the third or finishing coat, to be done with black paint as usual.

I am, Sir,

Your obedient humble Servant,

W. ANDERSON.

Master Painter of H. M. Dock Yard, at Portsmouth.

Portsea, Oct. 31*st*, 1806.

SIR,

AGREEABLY to the request in your letter, I have enclosed certificates relative to my new method of painting canvas; and I take the liberty of informing you of a method of obtaining from painted canvas, unserviceable, the whole of the colour laid thereon, and to do it at a very small expense. This I discovered since I last wrote to you, and I believe it will be of considerable advantage to government, who, for want of such a thought, have buried and burnt immense quantities of ships' hammock cloths, when found unserviceable, to prevent embezzlement from taking place. I suggested the idea to N. Diddems Esq. builder of Portsmouth yard, who communicated it to the honourable George Grey, Commissioner. I obtained leave to make an experiment, which I repeated thrice, and found that from one ton of painted canvas, unserviceable, I obtained, upon an average, four hundred weight of dry colour, in value to government nine pounds six shillings; the expense of the process not exceeding six shillings.

The colour may be obtained from old painted canvas,

at a trifling expense, by calcination.

This I effected by calcination, raking aside the ashes and sprinkling them with water, to prevent loss of paint through excess of heat. By passing the calcined matter through a fine sieve, it is perfectly prepared for grinding; it grinds well, possesses a good body for covering with, and dries well with a good gloss. Its increase of bulk, in comparison with common colour of equal weight, gives it the advantage of covering more work. The colours yielded by the calcination of different coloured canvas are as follows,

 viz.

viz. Canvas which has been painted with black paint only, produces a black colour. Canvas finished black, but which has had a previous red or yellow ground, will produce a dark chocolate colour. Canvas painted lead colour, will yield a good dark lead colour.

I am, Sir,

Your obedient humble Servant,

W. ANDERSON.

Testimonies to the superiority of canvas painted in the new mode,

Certificates, dated March, 1807, were received from the following persons, *viz.*

A. Stow, lieutenant and commander of the gun brig Steady, stating, that in the preceding month of October he had received on board his ship a set of hammock cloths, painted after the method invented by Mr. William Anderson, which had been constantly in use since the time above mentioned, and appeared fully to answer the end proposed, of rendering the canvas soft and pliable, and of preventing its cracking, or the paint peeling off, which in the old method had been a subject of much complaint.

John Pridy, lieutenant and commander of the Gladiator, and formerly commander of the Dapper, on which latter ship a set of hammock cloths, painted after Mr. Anderson's method, appeared fully to answer the end proposed.

P. F. Wyatt, oil and colourman, Portsea, stating, that he had seen canvas painted after Mr. Anderson's new method, which, after a trial of sixteen months, remained perfectly soft and pliable, the paint by no means cracking or peeling off, and that the gloss was retained, though it had been exposed to all weathers. He farther added, that he had seen the paint prepared by him from old painted canvas found unserviceable, and had worked and painted therewith; that it was, in his judgment, very good, and would answer either on canvas, wood, or iron.

and to the serviceableness of paint from old canvas.

Ns. Diddems, master shipwright, Portsmouth, dockyard, stating that Mr. Anderson had proposed to him to obtain, by calcination, from old unserviceable painted canvas, the paint which had been laid thereon; that such experiment

experiment was made, and four hundred weight of dry serviceable paint prepared from one ton of such canvas; that he had seen it when ground in oil and laid on work, when it appeared to possess all the properties of good paint, and had therefore been recommended by him to the Navy Board.

SIR,

IN answer to your letter of the 25th of April, in which you informed me, that the committee were desirous that I should furnish them with a sample of canvas painted in the old method, and another on my improved plan, I trust that I shall be able fully to comply with their requests. In the first place, I have sent a small sample of the residuum of the burnt canvas, fit for grinding in oil for paint, also a piece of canvas painted therewith, marked No. 1; another piece painted after the old method, marked No. 2; another piece painted according to my process, marked No. 3; and, lastly, a piece finished intirely with the new composition, marked No. 4; each sample having received three coats of paint. Upon examining No. 2, you will find it becoming from time to time more stubborn, in consequence of the paint hardening; and when a small ridge is formed in it, by pressing it between the finger and thumb, it will soon discover, that it is subject to crack, and by this means permitting the wet to enter it, will soon rot the canvas.

Samples sent to the Society.

The space of time proper between laying on the new preparation and the second coat ought to be one entire day; but if saving time is an object, the second coat may be put on the day following the first; for if the canvas is placed in an advantageous situation for drying, the composition will dry or harden so as not to rub off.

Time between the coats.

Canvas finished intirely with the composition, leaving it to dry one day between each coat, will not stick together if laid in quantities, as you will find by making experiments on the sample No. 4.

The canvas does not stick together.

Since the Navy board have given directions for ships' canvas to be painted according to my new method, I find, upon calculation, that I have painted upwards of twenty thousand yards since November last, a great part of which has

Requires little time to finish.

has not been hung up for painting and drying more than one week, as no more time could be allowed me, in consequence of ships sailing. My plan was therefore to lay on the composition the first day, to coat it the second day, and leaving one intermediate day, to finish it on the fourth. Three days were then allowed it to dry and harden, and when afterward taken down and folded together in cloths, containing sixty or seventy yards, they did not stick together.

Saving of three fourths of the labour in colour-grinding.

Having no means of giving information to persons concerned in grinding colours, so well as through the medium of the Society of Arts &c., I beg leave farther to relate how I have, for the last three years, saved the labour of three men out of four in grinding colours with the common mills employed for this purpose. One mill has ever been considered sufficient for a man to turn, whereas one man can now, with perfect ease, turn four mills; this is effected by placing two mills on each side of the winch, so close as only to leave room for the fly wheel to play between them. The spindles of each on either side are locked together by a small iron collar, with a pin passing through it. The distance of the mills thus paired from each, in order for the man's standing between them to turn, is two feet six inches. The distance of the arms of the winch screwed on the end of the spindles on either side is two feet two inches; the length of the arm is one foot six inches from the spindles to the bar across which the man clasps in order to turn.

Fly wheels.

Fly wheels at the extremity are impediments. Necessity was truly the mother of invention to me in this case, as I had great demand for paint, and I was not allowed men sufficient for the work in the common way.

Persons will scarcely believe, without seeing the experiment, the ease with which they turn. If a little extraordinary motion is first given them, and they are then left alone, they will continue to go round sixteen times; so that a man with one hand may turn them.

I am, Sir,

Your obedient humble Servant,

WM. ANDERSON.

SIR,

SIR,

I HAVE stated to the Admiralty Board the several improvements made by me in paint work, and in consequence thereof they have desired the principal officers of our yard to report to them on their merits. The officers, who have for more than twelve months past daily had the execution of them under their inspection, have recommended the same in stronger terms, and the advantages thereof, to the lords commissioners, beyond my statement. I have inclosed to you a certificate relative to the ship Hibernia, which arrived here the 12th of May last, and for which vessel I painted a set of hammock cloths, containing thirteen hundred yards of canvas, in June 1806, after my new method. Testimony to the superiority of the painted canvas.

I am, Sir,

Your obedient humble Servant,

WM. ANDERSON.

Portsmouth, Nov. 27, 1807.

The foregoing letter was accompanied with a certificate from Captain Hicks, and William Trounsell, carpenter of his majesty's ship Hibernia, stating, that the hammock cloths on board the said vessel were painted in June 1806, after Mr. William Anderson's method; that they were pliable, and did not crack, nor the paint peel off, and were, in their opinion, preferable to those painted in the common way.

SIR,

I BEG leave to trouble you with a farther certificate relative to my method of painting canvas.

I have also discovered a lead coloured paint, highly advantageous for all iron-work exposed to the weather, and preferable to that commonly made from white lead and black. The preparation is as follows:

Process for Lead Coloured Paint on Iron.

I take a fire shovel, and put a small quantity of common litharge thereon, and place it over the fire. I then take a small portion of flour of brimstone between my fingers, and scatter it over the litharge, when the same is sufficiently Lead coloured paint for iron.

sufficiently warm to give light to it. It is instantly converted to a blackish colour, which, when ground in oil, makes a good dark lead colour. It dries quick, gets remarkably hard, and resists the weather beyond any other lead colour. It will be extremely useful in the Ordnance department for painting guns.

I am, Sir,

Your humble Servant,

WM. ANDERSON.

Durability of the painted canvas.

This letter was accompanied with a certificate from J. B. Harrison, lieutenant and commander of his majesty's brig Red-Breast, dated Feb. 26, 1808, stating, that the hammock cloths on board the said vessel were painted by Mr. Anderson, in April 1807, and had since that time been constantly in wear, and exposed to the weather day and night; that owing to this new mode they had been preserved supple, without cracking, and that the paint adhered to them in a far superior manner to the former mode of painting.

XIV.

Account of the Royal Botanical Garden in the Island of St. Vincent. By DR. ALEXANDER ANDERSON.*

SIR,

State of the botanical garden at St. Vincent.

FROM my long silence you will conceive me either neglectful or ungrateful to the Society, but this is not the case. The reason is, I had nothing of consequence to mention relative to the garden, and it would be trespassing on your time, and interfering with matters of consequence by troubling you with trifles.

Although I have introduced a number of plants in the course of last year from different quarters, yet few of them are yet known to possess valuable properties, except some useful woods.

* Trans. of the Society of Arts, vol. xxv, p. 187.

I am

I am grieved to inform you, that I have lost one of my nutmeg trees; unfortunately the other, which prospers luxuriantly, turns out to be a male plant, consequently worth nothing. I blame myself in some measure, for this loss, by taking too much care of it, and not letting nature take her own way. Unluckily the war precludes any correspondence with Cayenne, or I would have replaced it from thence. The same cause has cut off all supplies from other parts. Through the medium of a gentleman who was here last year from Cuba, I expected to have had, before now, some of the productions of Mexico and adjacent parts of the continent, particularly myroxylon, or balsam of Peru; however, if I do not procure it through that channel, I have found out another from whence I have hopes.

Gomertur palm.

The Gomertur palm, which produces the material for cordage in the East Indies, is thriving here surprisingly, and, I think, might be rendered a valuable production to these islands. The mode of its producing the fibrous web, and the guard or protection surrounding, clearly point out, that nature intended it for the use of man; one tree produces an astonishing quantity. I think the fibres from the plants in this garden are stronger than the specimens I have seen from the East Indies. A small piece of the web, with its protector, I now transmit you. I have great reason to think that but few plants have been raised by the planters in the different islands, from the large quantities of seeds I have dispersed amongst them. The fact is, that no attention, except by a few individuals, is paid to any other plant but the sugar cane, and no other is in estimation with them.

Only the sugar cane attended to by the planters.

The bread-fruit, although one of the most valuable productions yet sent them, is neglected and despised, unless by a few persons. They say that negroes do not like it, and will not eat it, if they can get any thing else; but this is not really the case, as I know, and can declare from experience, that the very reverse is the fact, when once they are a little accustomed to it. The fact is, that the planters hate giving it a place on their estates, as they regard it as an intruder on their cane land, and they dislike any other object

They even dislike the bread fruit tree,

and are in every respect negligent of future benefit.

object but canes. As to futurity, they think nothing of what may be the wants of themselves or negroes three or four years hence. Even their most valuable mill-timber, than which nothing is more daily wanted by them, they are constantly destroying instead of preserving. They import it at an exorbitant rate, and the importation is precarious. With proper economy and management, there are few necessaries for themselves or negroes, but which might be raised on their own estates, instead of importing them from America, unless it be lumber, and, probably even that might be done in time in the back, cool, and mountainous situations. I am trying what may be done from the pine tribe.

Cinnamon.

I am happy that many are now paying some attention to the cinnamon, as the demands on me for the plants are frequent, which I impute to the specimens of it which I have shown.

Black pepper.

The black-pepper plants have not yet produced; I have them in plenty, and am trying them in various situations, and can easily increase them by cuttings; unluckily I can procure no information as to their culture in the East Indies, or of the soil or situation in which they thrive best.

Cloves.

I send you some more cloves, the last year's produce of two small trees; next year I expect from several others: you will also find inclosed a lump of gum resin from cochola odorata. As it issues in large quantities from wounds in the bark, it might be procured in plenty from Trinidad, if found useful. Trees of it, of enormous size, are abundant there. Other specimens of terra japonica would have been sent with some other articles, if all my attention had not been engrossed about the late addition to the garden: the same cause has prevented me from excursions to other islands for larger supplies of plants. I remain, with most sincere regard.

Sir,

Your obliged and obedient Servant,

ALEXANDER ANDERSON.

SIR,

SIR,

SINCE your last letter to me very little matter interesting to the Society has occurred, and few acquisitions made to the garden subservient to medicine or commerce. War interrupts correspondence in natural history as much as speculations in commerce. Correspondence interrupted by the war.

For 18 months past I have had expectations of some useful plants from Mexico, and other Spanish colonies in that quarter, by the way of Cuba, but thence the transportation must be circuitous by North America, and after that subjected to loss and interruption, before they can reach St. Vincent. I have therefore given up all hopes while the war continues.

As the Society may be desirous to know the present state of the garden, I have transmitted a catalogue of the variety of plants it contained on the 24th of September last: there are many more from different quarters received without names, or those that are known by the aborigines, and I cannot arrange them until they flower. I am, with great respect, Catalogue of plants.

SIR,

Your most obedient humble Servant,

ALEXANDER ANDERSON.

The catalogue alluded to, which is dated September 24, 1806, enumerates 67 commercial and medicinal plants; 49 esculent; 101 medicinal; and 63 economical: 76 valuable woods; 88 fruits; and 929 curious or ornamental exotics. There are likewise many others, which, not having flowered in the garden, cannot be ascertained.

XV.

Query on Accidents frequently happening to Dies with which Medals are struck. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR.

READING the life of Rambert Dumarest in a French periodical publication, I was struck with the following remark.

"To

Dies of medals very liable to be broken.

"To the specimens of Mr. Dumarest's talents I ought to add the medal on the peace of Amiens, the execution of which was awarded to him after a public competition. Unhappily, we are informed, one of the dies was crushed under the mill. This is a very frequent accident, and discouraging to the artist. Dumarest experienced it in a very vexatious manner; he was obliged to make almost every die he executed over again, and for one of his medals this happened no less than eight times following. The causes of it deserve inquiry. No doubt they depend on the quality of the steel, the care with which it has been forged, its temper, and the influence of the atmosphere; but still more on the mills, and the care taken in coining."

Are there any means of preventing this?

As I cannot gain any instructions on this point from the books I have at hand, and have no opportunity of acquiring information from any artist, I should be happy to be informed through the medium of your Journal, whether there be not means of preventing this.

It appears, that Dumarest was in England some time, and employed in the manufactory at Soho by Mr. Bolton, so that he must probably have been acquainted with many precautions taken there against an accident, to which it might be supposed the dies would be particularly liable, under the pressure of Mr. Bolton's powerful machinery.

I am, Sir,

Your very obedient humble Servant.

C. O. T.

SCIENTIFIC NEWS.

French translation of Archimedes.

IT is somewhat remarkable, that the works of a mathematician so justly celebrated as Archimedes has never been translated into any modern language till lately. I believe his Arenarius is the only tract we have of his in English; but I am informed a French translation of the whole of his works, that have reached us, has lately been published at Paris.

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Stratford, Printer, Crown-Court, Temple-Bar.